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BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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THE HOT-SPRING PROBLEM

BY ARTHUR L. DAY

(Address as retiring President of The Geological Society of America)

On an Anniversary occasion such as this it is appropriate to review certain perennial problems which have long been the subject of geological study and which do not obviously admit of immediate or definitely final solution. The hot-spring problem, for example, has been a subject of active discussion throughout a century or more, and opinions concerning the magmatic or meteoric origin of hot-spring waters are still highly controversial. Possibly its continuing indeterminate status is due to the fact that direct observation is possible only at the surface while the vital relationships are below ground and for the most part completely screened from direct observation. There are no convenient mine-shafts in the hot-spring country. Moreover hot springs are evanescent mechanisms in geologic history which leave no permanent exposures from which the sources of energy and water supply may be confidently traced.

It happens that this particular subject of study was beautifully presented to the Geological Society of America by its then retiring President, Dr. Arnold Hague, at its annual meeting in 1910, now 28 years ago. At that time Dr. Hague offered his latest conclusions from his own field studies in Yellowstone Park, which had been begun in the early 1880's and continued during most of the short summer seasons for nearly a quarter of a century. Associated with him as assistants or otherwise were other well-known names, notably the chemists Gooch and Whitfield, the physicist Hallock, and the geologists Iddings, Weed, and Jaggar. The mere mention of these names is sufficient to indicate the scope of the inquiry planned by Hague, and the time interval over which these investigations extended will indicate the persistence with which it was pursued. The hot springs of Yellowstone Park were hardly known to white men before 1870, so that the investigations here referred to not only indicated a prompt recognition of the outstanding importance with which the problem was then regarded but represented the most elaborate plan for hot-spring studies of which we have any record in America or elsewhere.

Perhaps it is pertinent to say here that Dr. Hague's investigation was never finished, and at the time of his Presidential address before the Society in 1910 there still remained in the United States Geological Survey

a mass of manuscript notes of uncompleted and uncorrelated observations which never have come to publication. The volume, Part II of Monograph XXXII, *On the Geology of Yellowstone Park,* which had been published by the United States Geological Survey in 1899, was really intended to be only the setting for a final volume (Part I) of definite conclusions based upon physical measurements and chemical studies of very comprehensive character. The published volume described in full detail the geologic history of the region immediately surrounding what is now Yellowstone Park, but the only conclusions publicly presented by Hague regarding the remarkable hot-spring activity within the Park itself are to be found in this Presidential address of 1910.

I do not think we need be concerned here with the details of hot-spring theory which had been developed in Europe in earlier years. These discussions derive largely from studies of the older formations in which the relationship of the hot springs to antecedent volcanism has become considerably attenuated, and the actual amount of energy involved is now relatively small. Certain of these will be mentioned as this discussion progresses, but it is significant that Hague in his address attached little weight to such discussions where the source of energy was remote and the details of its progress to the surface obscure. Although there are two great hot-spring regions in the world (Iceland and New Zealand) comparable with Yellowstone Park in the number and variety of the hot-spring phenomena which may be observed there—and these have been somewhat more intimately studied since Hague's time than before—still I think we may follow Hague in concluding that most of the phases of hot-spring activity in its development from volcanism may be followed without essential loss of continuity in the Yellowstone Park area.

Suppose we consider the major conclusions reached by Dr. Hague following his long field studies. He says:

"Nearly all regions where such waters issue from the ground on an imposing scale appear to have been at one time or another scenes of eruptive energy. In so many instances has this been shown to be the case that thermal activity and volcanic manifestations have come to be regarded as associated phenomena. It by no means follows, however, that the original source of all these waters was, geologically speaking, deep-seated, and by a large school of geologists it has never been so regarded. In recent years the results of several suggestive researches have been published, in which the position is taken that superheated waters issuing from igneous rocks are primitive in their origin—that is to say, they are derived from great depths in the earth's crust and are brought to the surface for the first time by volcanic forces."

Hague makes no concession to this view but concludes:

"Several years ago, after a study of the region [Yellowstone Park] under the auspices of the United States Geological Survey, . . . I stated the conclusion that the waters of these hot springs and geysers were essentially meteoric waters that had penetrated downwards a sufficient distance to attain an increased temperature, only to be forced again to the surface by ascending currents."

We may bear these two major conclusions in mind:

(1) Hot-spring activity is intimately related to volcanism, but (2) the waters now participating in hot-spring activity are of meteoric origin.

Hague then proceeds with his usual clearness and skill to marshal the geological and chemical evidence in support of these views. He describes the Yellowstone Plateau as a rhyolite mass, from 1000 to 2000 feet in thickness, formed by a succession of flows covering the floor of a great valley included between its oldest barrier, the Gallatin Range and the more recent (Eocene) Absaroka Range, forming the western and eastern boundaries of the Park Plateau respectively. The immediate source of these rhyolite flows is somewhat uncertain today, but all observers appear to agree that they followed in rather rapid succession one upon another without intervening beds of ash indicating explosive activity or erosion effects indicating long pauses between the flows. Hague also points out very pertinently, following Iddings' intimate studies of the rhyolites, that their physical structure is such that it would readily permit the circulation of surface waters in seams and joint cracks. Chemical analysis then adds its supporting evidence in establishing the identity of the minerals actually found in solution in the springs and their intimate relationships to the composition of the rhyolite.

Another phase of Hague's investigation definitely fixes the beginning of the activity of thermal waters in the Yellowstone rhyolite plateau. He observed that the massive horizontal beds of calcium carbonate, reaching 250 feet in maximum thickness, forming the summit of Terrace Mountain, and obviously formed by deposition from the thermal waters, bear upon their surface numerous boulders of glacial origin clearly indicating that hot-spring activity antedated the latest glacial visitation. He adds:

> "It is a fair assumption that if these thermal waters were issuing through rhyolite at one locality (Terrace Mountain) in pre-glacial time, similar hot waters and gaseous emanations should have reached the surface at other points on the plateau. . . . In regard to the age of the hot springs it is reasonable to conclude that thermal waters were as active at the close of the rhyolite extrusions as at any subsequent period."

He places the rhyolite intrusions definitely in Pliocene time, which would date the beginning of hot-spring activity from the close of that period, and adds:

> "In my opinion they have never ceased to be active and have only varied in intensity from time to time."

In discussing the character of the hot springs themselves Hague recognizes at least three distinct varieties: (1) The waters carrying calcic carbonate in solution; (2) siliceous alkaline waters; and (3) siliceous acid waters, usually holding free acids in solution. He also recognizes the possibility of mixtures of these in the same spring group. The dominant

calcium carbonate in the Mammoth Hot Springs he ascribes to the fact that in this basin "the upper lava flows lie directly against inclined Jurassic limestones." He also calls attention to considerable differences in the silica content of the waters and notes that the acid springs carry free hydrochloric or sulphuric acid and run but little water.

Gases from the thermal springs were also studied in those years and the more important ones identified. The carbon dioxide, however, is assumed to be of atmospheric origin carried down by percolating surface waters.

By way of general deduction Hague concludes:

"The early waters are usually acid in composition and deposit ferric and aluminous salts. Occasionally they set free sulphur, derived from the decomposition of hydrogen sulfide. In time the openings through which they flow become broader, the waters themselves, free from hydrogen sulfide, become clearer and neutral and at last issue as siliceous alkaline waters. Underground reservoirs are excavated and become sources of hot springs and, under favorable conditions, geysers. The geyser itself is simply a stage in the development of geological processes. . . . With the exception of arsenic and boron, which occur in minute quantities, all the elements brought to the surface in solution by the thermal waters of the Park have been found in the rhyolite. . . . In the Park country the vadose ascending waters do not appear to have been greatly affected by any primitive, deep-seated waters or their contents."

With regard to the major factor in the hot-spring problem, namely, the manner in which heat is supplied to them, Dr. Hague is obviously reticent. He says:

"In the discussion of the circulation of descending and ascending waters almost nothing has been said in relation to the source of heat which raised the temperature of these waters. This is in part due to the fact that the problems involved are in a great measure distinct from those treated here . . . and in part because I know little about the matter."

It was against this background that Allen, Fenner, and Day, of the Geophysical Laboratory, began studies in Yellowstone Park in 1925, in the hope of adding materially to the data available for such discussions and thereby increasing the probability of reaching definite conclusions. We were interested to establish the source of the energy and, if it should prove practicable to do so, to trace the manner of its transmission to the surface.

In the strictly geological relationships Hague's data and conclusions were accepted without question for his survey was extensive and most thorough. Regarding the physics and chemistry of hot springs it seemed to us that the data available might be advantageously supplemented in several directions with the help of modern technique and chemical knowledge. Due to improved conditions of transportation in the Park it had also become quite possible in the interval to increase the extent of such studies considerably.

First and most obvious of the steps which seemed to be necessary to supplement Hague's observations was to arrange some means of placing

the springs and geysers under continuous observation throughout the year, bearing in mind that all Hague's observations as well as those of Peale before him were confined to short summer seasons. If our effort is to be directed toward a determination of the sources of heat and water supply it is plainly of the first importance to ascertain their distribution through the year as affected by surface temperature and rainfall. Winter temperatures in Yellowstone Park range down to — 40°F., and the precipitation during more than 6 months of the year is withheld from normal circulation in the form of snow and ice. Recognizing the pertinence of this inquiry, the Park management courteously arranged that experienced Rangers should be placed at our disposal during the summer season to learn the details of observation and measurement and afterward should be assigned to winter quarters in the Upper Geyser Basin. During each of 5 years (1925-1930) temperatures were measured weekly in a considerable number of the springs and geysers of the Upper Basin throughout the winter season, and occasional record was kept of water levels and of the time and height of geyser eruptions insofar as daylight permitted.

Speaking broadly of the results obtained in these winter surveys it now appears to be established that there is no difference to be noted by experienced observers in the behavior of the geysers during the winter season or in the amount of water discharged. The hot springs likewise show no pertinent differences in temperature during the winter months (Allen and Day, 1935, p. 242-248). In April many of the hot springs and some of the geysers are flooded with cold surface water from the melting of the accumulated snows of winter, during which of course some disturbance of surface temperatures results, but these appeared to have no disturbing effect upon the conditions of subsurface circulation. One conclusion appears to follow at once from this, namely, that, in the Upper Geyser Basin where the observations were made, the amount of meteoric water circulating below ground is so large that the water partially withheld from this circulation in the snow and ice of a 6-months' winter is inadequate to disturb it seriously. Neither does the freezing of several feet of ground surface produce any measurable effect upon the character or the amount of ground-water circulation or upon its characteristic temperatures insofar as can be determined from such surface observations.

Observations to the same purpose have also been undertaken at Mammoth Hot Springs where weirs were set up in all the streams carrying surface run-off from these springs and were kept under observation weekly for three winters. These measurements likewise indicate no appreciable diminution in the amount of surface water circulating in the Mammoth Hot Springs area or in its temperature during the winter season.

Out of these observations the conclusion seems to emerge that so long

as the annual rainfall remains substantially constant the relationship between heat supply and its distribution, and water supply and its distribution, is not disturbed by seasonal influence. Moreover it may be added that a gradually diminishing rainfall during the 5-year interval covered by these studies did not produce a corresponding change in any of the factors under observation.

In the matter of the ultimate source of the heat contributed to the hot springs and geysers Hague did not appear to entertain any doubt. His words cited above are quite definite: "Thermal activity and volcanic manifestations have come to be regarded as associated phenomena." Nevertheless he explicitly disclaims any wish to inquire into the state of the magma or the specific mechanism by which the heat is continuously supplied to the springs in such quantities and over such periods of time as he supposes them to have been active, namely, since the Pliocene. The hot-spring waters were believed to be meteoric and "not greatly affected by any primitive, deep-seated waters or their contents." Carbon dioxide, uniformly present, was believed to be supplied from the atmosphere.

Since the time when Dr. Hague conducted his extensive studies in Yellowstone Park considerable positive information has been gathered about the constitution of the more volatile portions of the magma. They have been successfully collected from liquid lava at temperatures above 1000°C. and also have been pumped from solidified lavas of many compositions by inclosing them *in vacuo* and subjecting them to appropriate high temperatures. The results attained by both methods are closely similar. The major volatile constituents of the magma, whether collected upon release at the liquid surface or after solidification has occurred *in situ*, are water vapor upwards of 95 per cent, carbon dioxide in the neighborhood of 2 per cent, and the remainder distributed between compounds of the more active elements chlorine, fluorine, and sulphur, of which the most important is probably hydrogen sulfide, and occasional traces of boron and arsenic. Supposing these ingredients to be discharged beneath a rock cover in a hot-spring region, the chief acid radicals will combine with the accessible alkalis and appear as chlorides, fluorides, and bicarbonates with smaller amounts of sulphates or sulfides, borates, and arsenates. These are the magmatic components which must be sought in the hot-spring waters, and when their presence there is established they afford conclusive evidence of the participation of magmatic emanations in hot-spring activity.

The first consideration, then, in fixing the origin of hot-spring waters is to determine the mineral content of these waters. This mineral content will in any event accurately reflect the composition of the rock through which the meteoric water has passed, but if, in addition, sulphates and

alkali halides, borates, and arsenates, which are found in the rocks only in traces, or not at all, are discoverable in the waters, then the evidence of the active participation of magmatic emanation appears conclusive. And if these crucial components are found to be regularly present in the hot circulating waters of a hot-spring region then the associated magmatic components, superheated steam and carbon dioxide, must be present as well. This altogether fundamental relationship has now been established for all three of the great hot-spring regions of the world—Yellowstone Park, Iceland, and New Zealand. Upon this main issue, therefore, we appear to have made definite progress beyond the point where Hague left it in 1910.

From this point of view of the established participation of volatile components of the magma in hot-spring activity it is appropriate to give further consideration to the mineral content of the spring waters and their classification into groups as begun by Hague early in the century.

It is quite possible to confirm Hague's classification of the three major types of springs. They are:

(1) Those characterized by a dominant content of calcium carbonates and bicarbonates, of which Mammoth Hot Springs is the outstanding type. This carbonate content is now readily explained, as already pointed out, by the fact that the rhyolites here lie against Jurassic limestones which are gradually taken up in solution through the active agency of the carbon dioxide, now established as one of the two major ingredients of the magmatic emanations, and transported to the surface as bicarbonate in solution in the waters. Upon reaching the surface the excess of carbon dioxide is released, leaving carbonate in solution, of which a portion is now deposited on the surface as travertine. It is this deposition which has given us the beautiful travertine terraces at Mammoth Hot Springs and which is continuing today just as it must have done when the Terrace Mountain travertine was formed following the close of the Pliocene. There has been some diversion of the original channels of the percolating waters, but otherwise there seems no reason for believing that any essential change has occurred either in the character or the rate of this reaction and resultant deposition of the travertine.

In the present studies the effort was made to determine by measurement the rate at which such deposition is going on, and although a number of measurements were made of the thickness of the carbonate deposit upon wood blocks advantageously placed for the purpose, this was found to be quite variable (from paper thinness to 50 centimeters or more in a single year), depending apparently upon the local concentration of the solution, upon the temperature of the ground, of the atmosphere, and upon the turbulence of the emerging stream.

There is no reason either logical or observed for assuming any difference in the character of the original magmatic emanation here from that which supports the hot-spring activity in the other areas to be described. It merely happens that the presence of limestone locally gives to the carbon dioxide reaction with the limestone a completely dominant place in the mineral content of the hot springs emerging here.

(2) Another group, emphasized by Hague, includes the widely scattered "acid" or sulphate springs which together cover a greater area in the Yellowstone region than any other group. Reactions here are likewise determined by local conditions encountered by the magmatic gases but in a somewhat different way. These hot-spring areas occur on hillsides and other regions in which paucity of ground water is the important characteristic feature. This observation may be interpreted to mean that the magmatic gases in their passage through the overlying cover do not encounter ground water in these regions until they have approached very close to the surface, perhaps within considerably less than 100 feet, in consequence of which the acid radicals of the emanation assume the dominant role and lead the attack upon near-surface rocks whose temperatures are low. Where springs occur in these barren regions they are small, shallow, and turbid. The waters show an acid reaction; the sulphates appear in high concentration in the waters and form the dominant surface deposits. Fumaroles, large and small, are usually found in these regions, plainly indicating, in the cases where their temperature is high, that insufficient ground water has been encountered in transit even to condense the superheated steam of the original emanation. Where their temperature is below the saturation point some ground water will have been encountered but not sufficient to condense all the magmatic water vapor and so to form a spring.

Particularly interesting in this connection is the so-called Growler Spring in Norris Basin, which in times of abundant rainfall is a normal hot spring with rather abundant gases bubbling through it and a temperature appropriate to the boiling point of water at that elevation (about 93°C). Long-continued dry weather removes the participating ground water, and a fumarole results with a temperature of about 103°C. The fumarole is thus the limiting case where the steam of the magmatic emanation remains uncondensed and dominant until discharged into the atmosphere. This may not be interpreted to mean that the active chemical ingredients of the emanation have not been continuously active in the vapor-phase alteration of accessible soluble minerals throughout their passage from the magma through the overlying cover, for abundant evidence of this activity is found in the completely altered rocks later found by boring. Indeed the more active chemical reagents of the emanation

may become completely exhausted in this process, and the remainder when discharged into the atmosphere may consist of little more than the two dominant volatiles, superheated steam and carbon dioxide. An interesting case of the emergence of this final remainder has been found in the Karapiti fumarole in the North Island of New Zealand, which emerges upon a dry hillside with a deafening roar and a temperature of 110°C., but its output upon analysis revealed only steam and carbon dioxide.

With this group are included the so-called "Paint Pots", "Porridge Pots", and "Mud Volcanoes", all of which are hot springs of limited water supply and acid reaction in which the immediately subjacent surface rocks are undergoing active chemical attack from magmatic steam and acid gases to provide the "porridge" while the iron content of the rocks provides the "paint."

Subject to the same conditions of limited water supply are the considerable areas of gas-barrens, in which magmatic gases, more or less modified in passage from below, are escaping by seepage over considerable areas, sometimes with audible hissing sounds, and occasionally depositing free sulphur at or immediately below the points of issue. The conspicuous reactions here again are due to the surface oxidation of hydrogen sulfide to sulphuric acid upon contact with the atmosphere, of which the products are free sulphur and associated sulphates, resulting from sulphuric acid reactions upon rhyolite or rhyolite glass.

(3) The third group of hot springs distinguished by Hague includes practically all the deep, clear springs in the valley-bottom drainage which are characterized by abundant water supply, neutral or alkaline reaction, chlorides and carbonates in solution along with colloidal silica in considerable quantity, and usually depositing pure silica in amorphous form, gelatinous, opal, or in fine opaque grains, depending upon the amount of associated water and the surface temperature. It is from springs of this character that the silica sinter terraces are developed, of which the most distinguished example was the great "White Terrace" beside Lake Rotomahana in New Zealand, destroyed in the volcanic eruption in 1886. Such springs are almost always deep and often distinguished by the brilliant, bluish coloring of the water, which, together with their white sinter setting, gives to them their paramount public interest.

It is to this group of springs that the great geysers uniformly belong, for it is quite obvious that an abundant water supply and a closely integrated circulation are necessary prerequisites for this type of hot spring. Geysers after all are merely a special case of hot springs and can hardly occur until development has proceeded in such a manner and to such an extent that capacious chambers below ground have had opportunity to

form in which sufficient pressure may accumulate to provide for the intermittent and powerful discharge which characterizes these springs.

The actual progress of the reactions between the point of departure of the released magmatic materials from the magma through the rock cover to and through the ground waters of the deep circulation is a more intricate matter and requires some direct evidence from below ground in order to provide a full explanation of the surface relationships found. To attain this result it was deemed necessary in the Yellowstone Park to undertake borings, from the recovered cores of which some record of the continuing progress of active magmatic reagents might be traced. Accordingly with the consent and cooperation of Park officials two such borings were attempted, one in the Upper Geyser Basin in 1929, the other in Norris Basin in 1930. The Upper Basin is a region of abundant water supply, of deep, clear springs of generally alkaline classification. In this basin also the major geysers of the Park are to be found. Norris Basin, on the other hand, is a region of somewhat smaller water supply in which some of the springs are acid and turbid, some alkaline and clear, and geysers are not so numerous, so permanent, or so important.

The results of these borings have been discussed in detail by Fenner (1936). He finds in the first instance that temperatures increase rapidly downward in both regions, but much more rapidly in the region of smaller ground-water supply. The temperature in the Norris Basin at the greatest depth reached (245 feet) was 205°C., and the steam pressure at that depth, but measured at the surface, reached 300 pounds. In the Upper Geyser Basin, on the other hand, the lowest depth reached was 406 feet where a temperature of 180°C. and a maximum steam pressure of 57 pounds per square inch were registered. The rapid increase of temperature downward in both cases is perhaps a priori evidence of the presence of magma not too far below. The vast volumes of steam discharged at these high pressures and temperatures are also suggestive of a near-by magmatic source, but superheat cannot be expected at the surface in the presence of abundant surface water.

The mineral content of the cores has been interpreted by Fenner in a most illuminating manner. Without attempting to reproduce in detail his actual observations, the logic of his conclusions runs in substantially this order. The presence of sulphates, borates, and arsenates, which are not found in the neighboring rhyolite but are uniformly present in volcanic emanations, again confirms their origin in the underlying magma. When these emanations leave the magma and begin their percolation upward through the cover, vapors of the alkali halides are swept along in the current of steam and carbon dioxide. At the lower limit of ground water they enter into solution in quantities appropriate to their temperature and

solubility, the steam condenses and thereafter is added to the circulating ground water itself.

These hot solutions react with the adjacent rocks in a manner appropriate to the concentration of the participating ingredients. Carbon dioxide, for example, attacks the silicate minerals of the wall rocks and extracts bases. The resulting alkaline solution then removes silica in colloidal solution. Carbonates and bicarbonates of the alkalis are only very slightly volatile; hence this excess of alkali must have been acquired after the gases left the magma. This process, not accessible hitherto, appears to account quantitatively for the presence of extra silica and a portion of the alkali in the deeper circulation.

Fenner reasons that "If alkalis and silica are acquired in this manner, alumina in excess should be left." Analyses of the drill cores in Norris Basin show considerable excess of alumina, and clay minerals are found at all levels. In the Upper Basin these relations appear now to have reached levels below the lowest point attained by the drill.

But the analysis proceeds even further and with higher refinement. Among the clay minerals the presence of kaolinite is commonly attributed to the action of acid solutions and beidellite and its associate group to alkaline environment. In the Norris bore hole from a depth of 95 feet upward white clay replacing phenocrysts of feldspar proved to be for the most part kaolinite, and from that point downward to the bottom of the boring (245 feet) the clay was of beidellite type. The acid reaction is thus shown to be characteristic of the shallow surface circulation in this uppermost 100 feet, while below that the acid appears to be neutralized by the alkalinity of the bicarbonates. The relative depth of this boundary is probably fortuitous and variable, depending upon the depth to which surface waters circulate freely. In the Upper Basin a much deeper circulation of these ground waters is indicated than at Norris.

In considering these chemical relationships it appears to be true that the acid reaction is always dominant in a surface layer of varying thickness. Since the acid may be assumed to be chiefly derived from the oxidation of magmatic hydrogen sulphide by atmospheric oxygen it will be confined to those levels in which percolating surface water, carrying air in solution, dominates the situation.

One other important chemical reaction discovered by Fenner in the Yellowstone Park helps to explain the great preponderance of sodium over potassium in the hot-spring waters. He says:

"The two most important *rock constituents* in the issuing waters are silica and the sodium ion. These, in comparison with all other ingredients, are present in much more than their proper relative amounts. As for the sodium ion, the petrographic and chemical investigation of the cores gives convincing evidence that though the rocks have lost sodium the process has been chiefly the replacement of sodium in the

aluminosilicates by potassium supplied by waters rising from greater depths. The loss of sodium has therefore been made up by its equivalent of potassium. . . . Thus we find that though the issuing waters bring up silica in great relative excess over all other constituents except sodium, yet the rocks, as far as the drill hole has explored, are richer in silica than originally. Evidently waters rising from greater depths have brought in silica as well as potassium, and the silica in the issuing waters has not been leached from the rocks."

Thus the superheated steam and magmatic gases of approximately known composition, as they are released from the magma either through diminution of overlying pressure or crystallization of the magma, take their course upward through the seams and joint cracks of the rhyolite until the surface circulation of ground water is met with. Here steam condenses, yields up its latent heat and superheat to the ground water and mingles with it, while the carbon dioxide, hydrogen sulphide and the other active ingredients of the magmatic volatiles begin to enter solution at a temperature and pressure appropriate to the depth of this contact.

It is important to recognize that the steam as it emerges from magmatic solution is necessarily superheated and again necessarily it remains superheated until sufficient ground water is reached or sufficient cooling occurs to condense or saturate it. As we have noted above, when the progress of this superheated steam and accompanying emanation products reach the surface without substantial condensation by surface water or otherwise, a steam fumarole results. If but a small quantity of ground water is encountered the reactions of the acid radicals will probably dominate, as the rhyolite is actively attacked, and a shallow, turbid spring characterized by suspended sulphates will result. This condition obtains on slopes and hillsides where the available ground water is small in amount and its circulation shallow. Beneath the valley floors ground water is encountered by the magmatic gases at much lower levels and consequently at much higher temperatures than on the hillsides.

Although the extent of the sulphate areas in Yellowstone Park is greater than the extent of the areas occupied by the deep springs of the valley bottoms, their share of the ground water supply as measured at the outlet streams is only about one forty-fifth as great. Thus it is seen that, although the magmatic gases rising in the hot-spring areas are probably uniform in initial composition, the character of the springs is to a considerable extent determined by the amount of ground water locally encountered and the depth of its circulation.

Regarding the percentage of magmatic water contained in the hot springs of Yellowstone Park Allen (Allen and Day, 1935, p. 40) says:

"With the assumption that the spring water is steam heated, the only unknown factor required in computing the amount of steam necessary to raise the ground water in a shallow spring to any degree is the temperature of the steam and the water as they meet. If that of the steam is 138°C, the maximum temperature of superheated steam found at the surface in the Yellowstone Park, one kilo of

steam in cooling to 93°C and condensing at that temperature would give out (138—93) 0.45 + 543 = 563 kilogram calories, an amount of heat sufficient to raise 563/93-5[1] = 6.4 kilos of ground water to the same temperature. *In the resulting mixture the proportion of magmatic water would be 13.4 per cent.* Somewhat more would be required to make up for evaporation. The latent heat of steam is so great as compared with the heat given out in the process of cooling, that even if the initial temperature of the steam were 100° higher (238°C) the amount of magmatic water in a boiling spring would drop only to 12.7 per cent, neglecting the amount required to maintain evaporation."

Let us consider another phase of the situation. I have called attention on a number of public occasions to the fact that hot springs do not differ from cold springs merely in the fact that the waters encounter hot rocks in their passage to the surface instead of cold ones. If that were true the low heat conductivity of the rocks would quickly cause the heat supply to fail as the contact areas grew cold. In the Upper Basin, for example, is found the largest group (70 altogether) of great geysers known anywhere in the world. One of these, Old Faithful, plays with considerable regularity about once an hour and delivers upon the surface in each hourly period of activity an estimated 12,000 gallons of boiling water, none of which returns to replenish the supply. Since we have seen that this activity is practically continuous through winter and summer it is quite possible to compute the total annual output of boiling water from Old Faithful and the heat necessary to support it. The resulting computation yields the rather astonishing conclusion that something like 2 square miles of red-hot rock exposure would be necessary in order to support this one geyser for a single year if its heat were furnished solely from contact with hot rocks. Since Hague has told us that there is every reason to suppose that this hot-spring activity has continued undiminished since Pliocene time with no more than occasional changes in the channels through which the water reaches the surface, then obviously the 2 surface miles of red-hot rock would require replenishment annually through that interval in order to account for the activity of this one geyser. When we add to this the other great geysers and some hundreds of hot springs in the Upper Basin such an assumption rapidly becomes absurd. We are therefore dependent entirely upon the latent heat and the superheat of the magmatic steam to provide the energy for such a great group of geysers and hot springs as is contained in the Upper Basin. This energy enters the system at the lower zone of contact where the magmatic steam and associated gases enter the circulating ground water. Only in this way is an adequate supply of energy thinkable in terms of such periods of time.

At this point we have diverged considerably from Hague's conclusion in 1910 that "The vadose ascending waters do not appear to have been greatly affected by any primitive deep-seated waters or their contents."

[1] Mean annual temperature of unheated water in the Park.

The magmatic water is of course superheated steam in which, under prevailing pressures and solution conditions, the "critical temperature" is quite without significance. It is therefore this magmatic steam which provides all the energy and the volatile magmatic gases associated with it which determine the chemical reactions characteristic of the hot-spring region. This steam and associated gases mingle with the ground water in the zone of contact and thereafter become an integral part of it. The reactions of the associated waters thus charged with abundant energy and active reagents from below account for the metasomatic changes of which the adjacent rocks carry the record and eventually, on emerging at the surface, account also for the mineral content and temperature of the hot springs. As Allen has estimated, of the hot-spring waters of the Yellowstone area more than one-eighth is thought to be juvenile in origin.

This divergence of our ways away from Hague's major conclusion regarding the origin and distribution of the hot-spring waters necessarily leads to the reconsideration of another phase of the problem as it appeared in 1910. Hague was definitely of the opinion that the small acid springs represented an early development which must eventually be replaced by the deeper alkaline springs when the steam and acid reactions should have enlarged the approach channels of the water supply and the near-surface (acid) chemical attack should have abated. Indeed, that the small channels and basins should grow to larger ones under aggressive steam erosion is an altogether logical hypothesis. It is therefore pertinent to call attention to the fact that if our later reasoning is sound this may very well be true and still provide no disposition of the question of relative age.

The considerable array of observational fact which led us to the conclusion that the original composition of the deep-seated, magmatic emanations was everywhere constant and only the surface conditions were determined locally, chiefly through differences in the local supply of surface water, does not obviously lead to any conclusion regarding relative age. The limited ground-water supply upon slopes and hillsides cannot be materially altered merely by enlargement of passages by erosion. A fundamental change in topography appears to be necessary for that. Neither does it seem logical on the other side to assume that the deep alkaline waters of the valleys could ever be replaced by sulphate waters through ordinary processes of chemical evolution. Nevertheless it is quite conceivable that in the great basins the system of ground-water distribution might change from time to time as some channels become closed by deposition of silica and others are opened, and that such a local condition might result in so restricting a liberal water supply as to bring the zone of contact, where downward-percolating ground water meets the magmatic steam, nearer to the surface. In other words, the magmatic

superheated steam will again rise higher (locally) if or when less ground water is met with on the way. It would seem that such changes, if anywhere present, would occur through limitations of valley-bottom drainage rather than upon hillsides and, when present, must be very local in their effects.

As a matter of fact, there is one local area (Pipe-line Creek) in the Upper Geyser Basin in which the thick sinter deposits indicate a long period of earlier activity of the deep-seated alkaline type. These terraces are now more or less collapsed, and the very limited water supply now visible appears at the surface as shallow turbid springs of acid reaction. Such dislocated sinter terraces which can be attributed to no other origin than long-continued, deep-seated, alkaline activity, but which have now become dry and broken up and the deep springs replaced by a shallow and inadequate water supply, are much more numerous in New Zealand than in Yellowstone Park.

No definite field evidence of the converse transition (acid to alkaline) has been found in the literature nor has any been observed in the field. Such a case would appear to be more or less definitely barred by topographical limitations except in the hypothetical case of a low-level spring with locally limited initial water supply. A large increase in the groundwater supply is a vital prerequisite to change in this direction.

It is of course evident that these isolated observations do not return an answer to the main question, whether the shallow acid springs or the deeper alkaline ones are the older, and from the evidence available no positive answer seems attainable within the scope of our present knowledge.

Likewise unanswered remains the question of the participation of the algae in the deposition from springs of various mineral contents. That such deposition occurs between certain temperature limits appears to be demonstrated, but what relation such deposition bears to purely chemical precipitation is nowhere answered by the field observations thus far available. Studies of this subject have been made by different individuals in many parts of the world and in particular in Yellowstone Park by one of Hague's associates, Dr. Walter Harvey Weed, and later in our own time by Setchell and by several others. Nevertheless, whether the influence of the algae is selective or bears some other definable relation to the total mineral deposition in the hot springs appears never to have been determined.

One fact is here obvious, namely, that deposition through the intervention of algae can hardly occur above about 85°C. The springs which emerge at or near the boiling point of water carry no algae, and the integrity of their deposits is above reproach.

In concluding this rather summary review of the hot-spring problem we

may not pass over without consideration its most rare and most interesting "special case", namely, the geysers. That these are hot springs of alkaline type which differ in no essential respect in the relationship existing between the source of heat and the ground water appears to have been demonstrated satisfactorily both to Hague's generation and to ours. Hague describes them merely as a special case of alkaline hot springs and leaves it at that. Many other writers, including ourselves, have endeavored to probe into the determination of these special features and their explanation, but no solution hitherto offered has appeared to be altogether convincing. Nevertheless, it should be worth our while to discuss some of the details of such investigations and perhaps to point out certain limitations which have been recognized.

A geyser may perhaps be adequately defined as an intermittent, spouting spring. Within the scope of a definition of this breadth geysers of any magnitude may find a place beginning with the most insignificant and culminating, at least for the time being, in the great Waimangu geyser (now extinct) in New Zealand which played daily to heights above 1000 feet between the years 1902 and 1905. In the Yellowstone Park the highest measured eruption, originating in the Beehive Geyser of the Upper Basin, was accurately measured at 219 feet. It is possible that the great Excelsior Geyser, active in the 1890's in the Midway Basin, may have attained to greater elevations, but the records are obscure. Among the more trustworthy measured features the Imperial Geyser in the Lower Geyser Basin is known to have thrown out more than half a million gallons of water in a single 24-hour period, including two eruptions of about 4 hours each, but the height attained never exceeded 90 feet.

Many explanations have been offered of the mechanism of these spouting springs of which the best known was published by Bunsen in 1847. This was based upon observations of the Great Geyser in Iceland, and its author never claimed for it any other application beyond offering a plausible explanation of the behavior of the Great Geyser. Indeed Bunsen himself said that other geysers observed by him in Iceland seemed to be explained as well by an earlier theory propounded by Mackensie in 1811, also from observations in Iceland. In spite of the modesty of Bunsen's own claim for his theory it has, nevertheless, been very generally used to explain all geyser activity since that time without, however, attempting to correlate details.

Perhaps it should be said that Bunsen believed thoroughly in experimental demonstration, and in this case, in association with Descloizeaux, models were constructed to illustrate the behavior of the Great Geyser. It is from these models rather than from the examination of actual geysers in the field that the so-called Bunsen theory appears to have attained its

wide application. Almost every school boy has seen and understood these simple geyser models and so has come to accept the Bunsen theory without other effort at verification. Perhaps it is not strange under these circumstances that the theory should have been so little questioned.

The Great Geyser, according to description, and all the models since that time, may be described briefly as consisting essentially of a narrow tube of water, heated from below, with a shallow open basin at the top. There was of course no definite knowledge of how the water supply reached the tube below ground, but Bunsen assumed the existence of communicating passages in which water was boiling from volcanic heat. Accordingly all the models have been built with a small vessel in which water can be boiled, a narrow vertical tube tightly attached, and an open vessel at the top wide enough to catch the water and eventually to return it down the tube. Bunsen laid emphasis on the width of the tube and conceived that a very narrow tube would merely provide a continuously boiling spring, but if the tube were wide enough to permit considerable cooling at the surface and some circulation, and were slowly heated from below, a geyser should result. Bunsen had discovered by successive temperature measurements at various depths in the geyser tube, made during the quiet period, that the water temperature increased rapidly downward but always remained just below the boiling point for a particular depth and atmospheric pressure. Accordingly at a short distance below the surface the water temperature would be higher than the boiling point at the surface. A circulation which brought this hotter water quickly to the surface would therefore cause violent boiling. The sudden steam formation would throw out water and lighten the pressure upon the column below bringing successively deeper and so hotter layers into action. Operations were supposed to be started by large bubbles rising from the boiling water in the adjacent passages below. Only a slight upward impulse was necessary, he explained, to bring the entire column to ebullition. He offered no definite explanation of the manner of terminating the eruption.

Inasmuch as a geyser is by definition an "intermittent" spring and the above explanation makes no attempt to explain the intermittency which is its one outstanding feature, it seems somewhat surprising that "Bunsen's theory" should have received such unqualified and general application. It remained for H. O. Lang in 1880 to point out this limitation and to suggest details with which to complete the picture, but neither Lang himself nor a considerable number of successors in the field have received appropriate credit for supplying this vital omission. Perhaps this limitation failed to become obvious because the models built everywhere to illustrate the geyser action were all provided with a very limited supply of water and carried a catch basin at the top, which presently returned

erupted and air-cooled water down the tube, condensing the steam in the boiling vessel and automatically terminating the eruption. Continuing heat from below eventually reheated this same cooled water, and the operation was repeated. A continuing water supply appears never to have been provided in the models.

Now it happens that the geysers of Yellowstone Park very rarely have a catch basin at the top, and all the water thrown out during the eruption is permanently removed from the system. It is therefore immediately obvious that neither Bunsen's theory nor the models constructed in support of it are of any assistance in explaining geyser phenomena in Yellowstone Park. Take away the catch basin from the top of a geyser model and it ceases to simulate a geyser and returns to the simple dignity of a tea kettle with a long spout. Indeed such models seem to have little or no bearing upon the general problem of intermittent spouting springs. Moreover, there are geysers in the Yellowstone Park such as Oblong, Sapphire, Great Fountain, and Artemisia which do not erupt out of a narrow tube at all but rather out of a great bowl of near-boiling water as wide as it is deep. To such conditions the facts adduced by Bunsen and the explanations offered by him find no obvious application. In such great bowls the circulation is usually such that there is little or no difference in temperature between top and bottom. The water remains at or near the boiling point continuously, and in all the above cases the bowls are not emptied during or after eruption.

To set up anew an explanation adequate for all the phases of geyser activity observed in Yellowstone Park is certainly not a simple task and perhaps must await further and much more careful study of detail than has hitherto been accorded to this problem. There is great diversification of phenomena among the different geysers within the Yellowstone Park. Some are susceptible of simple explanation such as might be offered by the operation of the simple geyser model. Others are so far removed from this that few or no details could find application there.

Actual relations below ground are not accessible to direct observation or to approach by boring. Nevertheless there are certain assumptions which it seems possible to make today without encountering serious question. The source and the supply of heat, for example, like that of the other hot springs is undoubtedly continuous and practically constant for each geyser. The water supply is mainly meteoric as in the case of the other hot springs, heated and supplemented by the condensation of magmatic steam or superheated water derived from steam encountered farther down. Water circulation in the geyser areas is of the deep-seated alkaline type, precipitating silica which usually forms the lining of the geyser orifices and, more often than not, gradually builds about them a sinter

dome which definitely turns aside the falling water thrown out at each eruption. It also seems to be a matter of general agreement today that underground chambers, one or more, as the case may be, are necessary in which to accumulate a supply of discharge water and the steam pressure required to provide the eruptive power. To these chambers circulating ground water must be able to gain access against the accumulating steam pressure within. This may be assumed to mean that the pressure inside such chambers is quite variable, being lowest at the close of an eruption and highest just before the following one, and in consequence that the cooler ground water enters the chambers most easily and in greatest quantity during the period of lowest pressure, and the inflow gradually diminishes, perhaps to zero, when the pressure within is at its height. One dead geyser chamber has been found in Yellowstone Park and one in New Zealand, both of which show a number of very small side openings which may have admitted water or steam, or both, at appropriate stages of the internal pressure. Both chambers are otherwise lined with silica sinter and are reasonably tight. The pressure necessary to an eruption is obviously somewhat in excess of the weight of the overlying water column plus the weight of the atmosphere.

Perhaps the above might be considered necessary conditions upon which reasonably wide agreement would be accorded, whatever the size or shape of the opening, the latter merely serving to determine the surface features of the display when it occurs. There is one phase of activity common to many geysers which can hardly be explained otherwise than by a reasonably tight steam and water chamber below ground of considerable capacity. A number of geysers, including Old Faithful, for several minutes before a major eruption, develop periodic surges of overflowing water, usually of increasing intensity and volume, which can hardly be explained in any other way than as the result of a momentary and partial condensation of steam in a reasonably tight chamber against which the overlying water column is cushioned.

It is also a fact of observation that most of the large geysers of the Park disperse an amount of water during eruption many times greater than the volume of their accessible tubes. One or more storage basins of considerable size are therefore necessary to account for the volume of the visible eruptions. A geyser like the Giant, for example, erupts from a tube only 22 feet deep, 7 feet in diameter, and with a flat bottom. Nevertheless, its eruptions reach a height of 150 feet and often last for more than an hour. The water enters this tube from the sides less than 15 feet below the surface.

More difficult to account for by simple explanation are the variable periods of eruption and the variable time intervals between eruptions.

The Giant, for example, to which reference was just made, has been known to erupt on successive days and again to rest quietly for three weeks without eruption. The Beehive in recent years has erupted only two or three times in the summer season during which it is under observation, whereas in Hague's time it erupted at shorter intervals varying between 8 hours and 8 days. Old Faithful, as has been stated, erupts rather regularly at intervals of about one hour and five minutes, though when under continuous observation this interval is found to vary between half an hour and one and a half hours. Supposing the heat to be continuously supplied and the water supply to be reasonably constant throughout the year, as has already been demonstrated, it is a little difficult to account for these variations. It is just possible that an explanation may be found in a nice adjustment between maximum steam pressure within the chamber and the overlying load, which encounters no disturbing factor sufficient to release it for a longer or shorter period. This possibility is suggested by the fact that certain of the geysers may be set in activity arbitrarily by vigorous stirring at the surface or by dipping out a few pails of water or by the addition of a small quantity of soap, factors so small in terms of the total energy impounded that no other conclusion suggests itself but that we are dealing with a rather precise balance between two forces requiring a very small makeweight to upset it. Geysers that respond to the action of soap usually do so within 2 or 3 minutes after the soap is added—provided sufficient time has elapsed since the last eruption for full recovery.

These factors taken together comprise substantially the extent of our knowledge of the mechanism of geyser action today. Our observations are quite obviously incomplete, so that it is yet hardly possible to dignify these details by the name of theory, but we may perhaps venture to hope that at the end of the next 25-year cycle geysers will have found a reasonably complete and adequate explanation.

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A REVIEW OF RECENT PROGRESS IN REPTILIAN PALEONTOLOGY

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Sixteen years have elapsed since Matthew (1) made a report to this Society on the progress of vertebrate paleontology. These 16 years have been a fruitful period in the science, and it therefore seems appropriate on this occasion to take inventory again of some of the accomplishments. I shall confine my remarks to the Reptilia, the paleontological history of which has been my chief professional interest.

Terrestrial vertebrates as aids to geological interpretation have never been of more importance than they are today. For many years greater reliance has been placed on fossil mammals than on other vertebrates, but the great strides made in the past few years in our understanding of the fossil Reptilia have made their contribution to geological history of ever-increasing importance.

In the formative days of paleontology the Reptilia were known chiefly from fragmentary specimens. Our concepts of these animals were often incorrect and nearly always inexact. Of the conclusions of our predecessors, some have been swept away and forgotten, others have been modified, and some have been confirmed and strengthened by subsequent discoveries. More intensive collecting, supplemented by improved technique, has resulted in the assembling in our principal museums of large collections which are characterized by a quality of preservation undreamed of a quarter of a century ago. Complete articulated skeletons, embryos, skin impressions, eggs, and footprints all contribute to a more precise understanding of the animals to which they pertain.

In field work the utilization of the motor truck has been an important contributing factor to this progress. It has made distant and out-of-the-way areas more immediately accessible and has greatly increased the amount of time that could be devoted to prospecting and collecting. In reconnaissance work for the first time, the airplane has also come into use in large regions in searching out likely-looking collecting areas.

One of the outstanding developments of recent years, especially in American institutions, is the broadening of the field of search for fossil specimens. The Asiatic expeditions of the American Museum of Natural

History into Mongolia and China; the Scarrett expedition led by Simpson to Patagonia; the Stanley Field expeditions under Riggs to South America; the Walker Museum expedition under Romer and Miller to South Africa; the Yale North India expedition under Lewis; the Museum of Comparative Zoology expedition under Price and White to the Triassic of South America; and the University of California expedition under Camp to South Africa are some of the more important of these.

This assembling in American institutions of large collections of foreign fossils, collected by American methods, will exert a most beneficial influence and will bring about a fuller understanding of the distribution, development, and relationships of faunas. Although faunal study of small areas will necessarily precede, we may now look forward to the time when a comprehensive study of many groups from all parts of the world will be possible—and upon materials assembled in our own institutions.

I will now briefly review some of the concrete accomplishments of the past 16 years.

Paleozoic: No important discoveries have been made of Pennsylvanian land vertebrates in recent years, but in Permian vertebrates both collecting and research have been continued with an activity equal to that of the two previous decades. Permian time is of unique interest, as it witnessed the first great expansion of land vertebrates; in all probability it was then that mammals, birds, and the chief orders of reptiles originated. In America, Case (2) and Romer (3) have been the leaders in this work, and their activities, combined with that of their students, have effected great progress in our knowledge of this ancient vertebrate life. Romer's (4) study of the stratigraphic sequence of the Texas Permian has established provisional zones of approximately formational scope. His recognition that some of the earliest finds of Texas vertebrates had come from beds considerably below the lowest Wichita and hence were of Pennsylvanian age is a discovery of much significance. It should be reported that this same authority at the present time has a monographic study of the Pelycosauria in press.

The Permo-Triassic faunas of South Africa still engage the attention of a great number of paleontologists of whom Broom (5), Watson (6), Houghton (7), Van Hopen (8), Boonstra (9), Parrington (10), Broili (11), and Von Huene (12) have been most active. Few faunas anywhere have been subjected to more intensive research, as is conclusively shown by the collective contributions of these savants.

With the acquisition, by the American Museum, of the Broom collection of South African reptiles and the large collections in the Walker and

University of California museums from this same region, brought together by Romer, Miller, and Camp, the Permo-Triassic reptiles have a more direct significance to American paleontologists than ever before.

An entirely new field for Permo-Triassic vertebrates was made known by the discoveries of the Sino-Swedish expedition (1925-1931), in northern Tienshan and in Sinkiang, China. The animals so far discovered are represented by well-preserved specimens. These forms suggest correlation with the Karroo of South Africa and a possible land connection between the two continents in Permo-Triassic times. Papers descriptive of these materials by Young (13) and Yuan (14) are now appearing from time to time.

Triassic: Substantial progress has been made in the past 16 years in our knowledge of the Triassic vertebrate life of America. Case (15), Camp (16), Mehl (17), and Brown (18) have been the chief contributors to this progress. Our knowledge of the phytosaurian reptiles has been greatly advanced by the work of all these authorities, but the monographic study by Camp is worthy of special mention. Case's work in the Triassic of Texas has been of a pioneer nature and has produced most important results, not only in revealing new forms, such as the heavily armored *Desmatosuchus,* but in the discovery of materials of *Coelophysis* that now places this little-known primitive dinosaur on an adequate basis for comparison with the more highly specialized forms of later geological periods. The identification by Case (19) of a true nothosaurian in the Triassic of Wyoming is the first recognition of this group of marine reptiles on this continent.

The geographic range of the Triassic Ichthyosauria has been extended northward to Peace River in British Columbia through the discoveries of Sternberg. These materials have not yet been studied.

The discovery by Brown in the Chinle of Arizona of a nearly complete articulated skeleton, including the dermal armor of *Archaeosuchus* (18), which he regards as an ancestral crocodile is a most important addition to this fauna.

In Europe, von Huene (20) has been a persistent and voluminous contributor to our knowledge of Triassic life, not only of Europe but of Africa and South America. His contributions have had a very great influence on contemporary paleontology.

From southern Brazil, Romer informs me, the Museum of Comparative Zoology has 7 tons of materials collected by Price and White. It consists chiefly of Triassic rhynchosaurs and dicynodonts and is now undergoing preparation.

In Switzerland, Peyer (21) and other collaborators have published a

notable series of studies descriptive of the large assemblage of reptilian forms in the University of Zurich from the Triassic in the vicinity of Lake Lugano.

A discovery of much interest was Simpson's (22) determination that *Dromatherium* and *Microconodon,* long thought to be mammals from the Triassic of North Carolina, are actually reptiles. Thus we no longer have any Mammalia in the Triassic of this country.

Jurassic: The outstanding feature of progress in the Jurassic field of recent years is the dinosaur quarry in northern Wyoming, developed by Brown (23) for the American Museum of Natural History. Second only to the famous Dinosaur National Monument quarry in Utah, it is the most amazing concentration of dinosaurian bones discovered in recent times. An area of many hundred square feet was literally packed with dismembered portions of skeletons of several types of dinosaurs, but the long-necked, long-tailed, small-headed sauropods predominate. More than 4000 individual bones that completely filled a large freight car were collected. This assemblage has not yet been studied, but I am informed that several new forms are present, and, coming as they do from a new geographical province, there is every probability that this fauna represents a phase of Jurassic dinosaurian life new to this continent.

Mention should also be made of the finding of extensive deposits of dinosaurian fossils in the Morrison of western Oklahoma by the University of Oklahoma field parties under the direction of Stovall (24). More than 3500 individual bones have been collected, most of them of the genus *Apatosaurus.* The genera *Ceratosaurus, Camptosaurus,* and *Stegosaurus* are also reported as occurring here. The discovery is of interest in extending the geographic range of this dinosaurian fauna much farther eastward than any previously known occurrences.

Although practically all collecting in the Dinosaur National Monument quarry was done prior to the anterior limit of this review of progress, the scientific results have been appearing in more recent years. Memoirs on *Diplodocus* (25), *Camarasaurus* (26), and *Apatosaurus* (27) based on nearly complete skeletons are important contributions to our understanding of the skeletal anatomy of these giant animals.

In this connection the commendable policy adopted by Director Avinoff of the Carnegie Museum is worthy of mention. It is his plan in the handling of their enormous Dinosaur National Monument collection to publish a series of well-conceived monographic studies as expeditiously as possible and to dispose of their duplicate materials by exchange. Already many exchanges have been consummated with American and Canadian institutions. This procedure will greatly expedite the preparation of this

difficult material, thus making it more quickly available to those interested. No less than eight skeletons from this one quarry are now on display in the museums of this country, and others are under reconstruction.

A project planned by the National Park Service on this reservation promises to be of unique educational interest, showing dinosaur specimens in the original rock and on the original spot where found. All the preliminary work of preparing a display of dinosaurian skeletons *in situ* has been completed. It now remains to work the specimens out in deep relief on the sloping sandstone wall of the quarry so admirably adapted to such a display.

The authoritative memoirs of Hennig (28) and Janensch (29) on the Tendagura dinosaur collections from East Africa in the Berlin Museum have been appearing at irregular intervals. These provide a very complete knowledge of some half dozen types. Three of these, the armored *Kentrurosaurus*, the long-spined sauropod *Dicraeosaurus*, and the long fore-limbed *Brachiosaurus* (an American genus) have been excellently posed and mounted for exhibition. On the whole this fauna in its broad facies resembles that of the Morrison of this country.

The finding of a representative of a new family of small light-limbed carnivorous dinosaurs in the Navajo sandstone of Arizona is an important contribution by Camp (30) to the meagerly known transition fauna between the Triassic and Jurassic. This form is said to represent an intermediate stage between *Compsognathus* and *Ornitholestes* on the one hand and the coelurosaurians on the other.

Cretaceous: Less than a quarter of a century ago our practical knowledge of the Cretaceous Dinosauria of North America was confined to the Lance formation, as represented in one zone and in one small area in Wyoming. Such fragmentary specimens as were found in other formations at that time and thought to be distinguishable were nearly always the subject of much difference of opinion. With the discoveries in recent years there are now no less than five geological zones in the Cretaceous that have been or can now be characterized by extensive dinosaurian faunas founded upon adequate specimens, many of which are practically complete skeletons. Named in ascending order these are the Cloverly in southern Montana, the Mesa Verde in southern Wyoming, the Belly River and Edmonton of the Red Deer River region in Alberta, and the Lance in Wyoming, Montana, and Saskatchewan.

Today the known scope of the geographical limits of the Upper Cretaceous Dinosauria in North America extends from Peace River Canyon in British Columbia on the north into northern Mexico on the south and from the Great Plains on the east, westward into California.

The most notable contribution in recent years to our knowledge of the Cretaceous Dinosauria of America is the establishment of new faunas as a result of the expeditions of the American Museum of Natural History under the leadership of Doctor Barnum Brown. Reference is made to the remarkable series of skeletal remains from the Cloverly formation in southeastern Montana and to a collection from the Mesa Verde formation of southwestern Wyoming, from which previous to the past 2 years no dinosaurian specimens had ever been collected or described.

This discovery of articulated skeletons of armored, carnivorous, and camptosauroid dinosaurs in the Cloverly, all of which pertain to undescribed genera and species, and of the specimens of ceratopsian, hadrosaurian, iguanodont, and armored dinosaurs from the Mesa Verde, will fill long-existing gaps in the faunal succession of dinosaurian life of the Upper Cretaceous. When these collections have been studied and described they will form an outstanding contribution to the advance of our knowledge of the life-history and evolutionary development of dinosaurian life on this continent.

The exploration in 1937 of the Wasatch Plateau region of central Utah by an expedition from the Smithsonian Institution made a notable beginning in the development of a fauna for the North Horn formation of the Upper Cretaceous. This is also a new area for dinosaurian fossils, and the discovery of ceratopsian, hadrosaurian, theropod, and sauropod dinosaur remains here greatly extends to the westward the known geographical range of all these groups. The unexpected discovery of an articulated skeleton of a gigantic sauropod dinosaur (31) in association with typical Upper Cretaceous forms gives additional and convincing proof that these animals persisted into that geological period in North America.

The first discovery by Reeside in 1921 (32) of sauropod dinosaur bones in the Ojo Alamo formation, Upper Cretaceous of New Mexico, has been reluctantly accepted by some paleontologists for the reason that previously these gigantic animals were all supposed to have become extinct on this continent at the close of the Jurassic. Further evidence of the Upper Cretaceous age of the Utah sauropod is furnished by the specimen itself, for preliminary study shows at once that it pertains to the family Titanosauridae, a family established by Lydekker to include remains found in India from the Lamenta beds, the Upper Cretaceous of that country.

Mention should be made at this point of the Arundel Dinosauria fauna of Maryland, originally regarded by Marsh, Hatcher, and Lull as Jurassic, but which more recent study (33) has shown to be of Upper Cretaceous affinities. The presence of sauropodous dinosaurs in this assemblage was one of the most important factors influencing these earlier authorities into

believing they were dealing with a Morrison fauna. The more recent recognition of an unquestioned member of the Ornithomimidae, a strictly Upper Cretaceous family, associated with other fragmentary remains having Cretaceous affinities, coupled with the fact that sauropod Dinosauria are now known to have persisted into the Upper Cretaceous on this continent, leads to the suggestion that the Arundel may also be found to belong to the Upper Cretaceous portion of the geological section. At the present time, however, the paleobotanical evidence does not give support to such a conclusion.

Another phase of Upper Cretaceous paleontologic work in which progress may be reported is the development of faunas in outlying geographical districts. The Ojo Alamo-Kirtland formations of the San Juan Basin in New Mexico, through the researches conducted by the American Museum, Upsala Museum, and the National Museum, can now be quite certainly correlated with the Belly River of northern areas. Progress in this instance is unique in American paleontological history owing to the fact that a very considerable part of our present knowledge has been contributed by Doctor Carl Wiman (34), the Swedish paleontologist of the Upsala Museum, his researches being based on a collection of dinosaurian and chelonian fossils made by the elder Sternberg for that institution.

In the past few years, through the explorations of the National Museum and American Museum field parties, a considerable reptilian fauna has been developed for the Two Medicine formation in northern Montana. The precise relationships of this fauna to surrounding Upper Cretaceous areas is as yet not entirely clear.

Continued explorations of the Belly River and Edmonton formations by the Geological Survey of Canada and the Royal Ontario Museum parties have steadily contributed new and interesting forms of dinosaurian and other reptilian life to the already well established faunas of these formations. Numerous descriptive papers by Parks (35), Sternberg (36), Brown (37), and Gilmore (38) are the more important contributions in this field.

The monographic revision of the Ceratopsia by Lull (39) is of the greatest importance and it brings our knowledge of the horned Dinosauria practically up to date. It can also be reported that this same authority has a monographic study of the Hadrosauridae nearing completion. To students of the Reptilia this will be most welcome news, for no group of the Dinosauria has been in a more chaotic state.

The explorations in Mongolia, by the Asiatic expeditions of the American Museum of Natural History under the leadership of Andrews and Granger, constitute the outstanding single contribution to paleontologic

progress of recent time. This discovery in the Gobi Desert of extensive fossil deposits in which well-preserved specimens were obtained from formations of Cretaceous, Eocene, Oligocene, Miocene, and Pliocene age for the first time has revealed the vast paleontologic wealth of central Asia. The Reptilia formed a very important and spectacular part of these collections. No less than 13 new dinosaurian genera have been recognized (40), and fragmentary specimens indicate the presence of others. At least three dinosaurian faunas have been fairly well established, two of which, the Dajochta and Iren Dabasu (41), are of Upper Cretaceous age, and one, the Oshih, belongs to the Lower Cretaceous. All important groups of the Dinosauria are present—the Hadrosauridae, Protoceratopsidae, Nodosauridae, Ornithomimidae, Deindontidae, and the Sauropoda. The discovery of *Protoceratops* (42) opened a new chapter in ceratopsian history, since it constituted the first record of the group outside of North America and furnishes for the first time an ontogenetic record of a dinosaurian genus from the egg and newly hatched young through all stages of growth to the adults. This large series of specimens is now being studied by Brown and Schlaikjer.

In India, Matley (43) has amassed an interesting collection of Upper Cretaceous dinosaurian materials. Members of the Coeluridae and Ornithomimidae are recognized here for the first time. Von Huene recognizes in this collection three genera of sauropods that are common to Patagonia. The close relationships of these animals he regards as supporting evidence that Patagonia and India were connected in Upper Cretaceous time by way of the Antarctics, Australian region, and the Sunda Archipelago. The extended study of the South American Sauropoda by Von Huene (44) is a work of the highest authority and it gives us a most comprehensive publication on this group from that continent. All these animals he considers Upper Senonian.

In China, since the first discovery of dinosaur remains in 1929, two important fields have been found. From the Cretaceous of Shantung, Wiman (45) has described the very complete sauropod skeleton *Helopus zdanskeyi* and other dinosaurian remains; and Young (46), of the National Geological Survey of China, has described sauropod and armored dinosaurian remains from Sinkiang. Should more modern collecting methods be used both these fields give promise of producing excellently preserved specimens. The Shantung dinosaurs are considered Lower Cretaceous, and Morris (47) has suggested correlation with the sauropod horizons of the Gobi.

In Mongolia, on the middle course of the Amur River, a new area for Upper Cretaceous dinosaurs was discovered by Russian paleontologists. A theropod and two hadrosaurian genera were described by Riabinin (48),

two of them on very scanty materials. It is of sufficient interest to mention that a specimen of the trachodont *Mandschurosaurus* now forms a very creditable mount (49) in the Museum of the Geological Committee in Leningrad. Riabinin has also reported the finding in 1924 of Cretaceous dinosaur remains, presumed to be hadrosaurian, in Turkestan, but I have failed to find mention of the discovery elsewhere.

In reviewing the many widely separated fossil fields found in Asia during the past few years, one cannot help but be impressed with the great possibilities of that country for future paleontological research.

In Australia, where only a few years ago few fossil reptiles were known to exist, Director Longman (50), of the Queensland Museum, has made known in a series of descriptive papers the occurrence of both Jurassic and Cretaceous sauropod dinosaurs and other extinct reptiles.

Though the Dinosauria, owing to intensive study, have become the most significant vertebrates of Secondary time, progress may also be reported in our knowledge of some of the less conspicuous Reptilia.

The Crocodilia have been the subject of numerous preliminary papers by Mook (51), and I am informed that his monographic study is reaching the final stages. The wide distribution of this group, both geographically and geologically, renders a full knowledge of it of interest to both paleontologist and geologist, and it is with pleasant anticipation that we look forward to the appearance of the completed work.

Monographic revisions of the fossil lizards (52) and of the fossil snakes (53) of North America by Gilmore afford a useful compendium of what is known of these groups up to the present time.

Although regarded with little favor by many paleontologists, nevertheless progress may be reported in our knowledge of the fossil turtles. Their geological and geographical range have been extended, and the discovery and description of new forms and complete specimens have contributed much to the anatomical knowledge of many species known previously only from fragmentary remains.

The realization that in evolutionary studies actual sequence in time is as important as the degree of completion in structures has brought about a marked trend in recent years among vertebrate paleontologists toward determining the precise position of specimens in the field and toward adoption of the latest advances of stratigraphic geology. These more exact geological records have enabled them to define zones and to differentiate faunas in much more detail than ever before.

On the anatomical side great progress has been made, because of the more complete quality of materials, and this perfectness of structure has stimulated researches of a high quality.

While it is still necessary to emphasize the limitations of our knowl-

edge of the Reptilia, I feel that it can be predicted with some degree of assurance that it will soon be possible to determine from the evolutionary stage of development of a reptile its proper position in the geological sequence, although this knowledge will probably never reach the degree of refinement attained with mammalian structures.

In reviewing the apparent trend of recent advances in vertebrate paleontology, I am greatly impressed by the spirit of cooperation and good feeling that prevails between the staffs of the various institutions. The loan of specimens, the turning over of entire collections to those engaged in an extended research problem, and the ever-prompt response to inquiries for assistance are a beneficial and inspiring influence and most important factors in the progress of the science.

In summing up this necessarily brief review of progress the results attained appear as significant as any similar period in the history of vertebrate paleontology. In the widespread exploration, in the discovery of new forms and faunas, and the volume and high quality of the published results, it has been a period of unusual activity with a wealth of output. To the future we may look forward with optimism, for the large collections of unstudied materials already in hand and the extended studies under way promise much for future progress not only of reptilian but of all paleontology.

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THE RISE OF PHYSIOGRAPHY

BY NEVIN M. FENNEMAN

(*Anniversary Day Address*)

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INTRODUCTION

It might be expected that the earth's surface would be studied before the deeper crust. This may be true if the word "studied" is not used critically. That the surface would be observed, talked about, written about, and even to some degree *thought* about, is a fairly safe expectation and seems to agree with the historical facts. When it comes to what may be called study in a scientific sense, we find that things taken for granted when civilization was young are about the last things to be investigated critically.

The result was that mineralogy, paleontology, and even microscopic petrography began their careers as critical sciences in advance of systematic physiography. It was only when present-day processes became the key to unlock the past that critical study was really demanded. This was not much before the nineteenth century. And it was not until the late decades of that century that topographic forms came to be regarded as subject to systematic evolution. The concepts of geomorphology in the early nineteenth century were surprisingly like those of the Greek philosophers.

It should be stated here that the term *physiography*, as most often employed by geologists, has come to be the practical equivalent of *geomorphology*, the science of surface forms or topography. It has also had a broader definition but that is not of interest here. Since the interest in earth forms is largely in their explanation or genesis, the scientific

study of agencies and processes is also involved. Hence no sharp line can be drawn between physiography and dynamic geology. Whether a man is studying one or the other must be determined by what he is after, not by the data he is using. It may seem to you as I trace the earlier stages of physiography that some of my illustrations pertain quite as much to dynamic geology or paleontology or some other line. And this is true. It is the nature of science in its earlier stages to be undifferentiated.

KNOWLEDGE IN THE ANCIENT WORLD

Knowledge of this subject as it existed in the ancient world comes down to us through the Greeks, who, beside being thoughtful observers, were travelers and learned some things from Egypt and other countries. Herodotus in the fifth century B. C. saw marine shells, not only in the Nile valley but in adjacent uplands. Knowledge of such facts was common enough among Egyptian priests and dated from an unknown past. Their true explanation and their bearing on the ups and downs of the surface did not escape Herodotus. That similar beliefs would imperil liberty and even life 2000 years later in Christian Europe would have been quite beyond the Greek understanding.

For the Greek and Roman world the question of the relation of sea and land was cleared up by Strabo. At about the beginning of the Christian era, this Greek, living in Rome, argued the question at length and painted a modern picture of the earth's surface rising and sinking in segments, submerging and emerging, accumulating sediments at one time and eroding at another. It is not to be assumed that this concept became universal, even among philosophers, but Strabo was high authority, and no State or Church had any interest in regulating his conclusions.

Among the ancients, as in modern times, earthquakes were associated with deformations of the earth's crust. If Greek scholars spoke of the deformation as resulting from earthquakes, whereas we reverse the order, it is not necessary to assume any great inconsistency. In both cases the two phenomena are associated, and it was not till the rise of modern seismology (Mallet's theory of elastic waves) that much advance was made. On this point Aristotle affords a curious combination of fancy and popular beliefs about winds, raging in enormous subterranean caverns. As for volcanoes, the true nature and origin of Mount Vesuvius was explained by Strabo, who died 50 years before its first recorded eruption.

The ancient world was curiously unable to see that atmospheric precipitation is quite sufficient to account for springs, rivers, and other forms

of fresh water. Indeed the supply of springs remained something of a mystery until 1674. Harvard College was 38 years old when a French lawyer, Pierre Perrault, published the first demonstration of the sufficiency of precipitation to account for flowing waters.[1] Aristotle would have spring waters produced by condensation in those mythical subterranean caverns, which figured so large in ancient and medieval science. The work of rivers was better known than their origin. That streams make their valleys was not the popular belief but was more than once set forth and was probably as clearly understood in the year 1 as at any later time before the nineteenth century. As for fluvial deposition, the Nile was too near to allow ignorance on that question. The statement that Egypt is the gift of the Nile is attributed to Herodotus, but the underlying idea must have been very old.

With such modern concepts as the foregoing (the list is by no means complete) and with a vision of constant change, it would seem that the Pythagorean school of philosophy was equipped to decipher geologic history, yet neither Greeks nor Romans seem ever to have attempted a consistent story of progress, either biological or physical, or even to have conceived of the earth as having had such a history. It was indeed seen that a new earth was constantly being fashioned, or an old earth changed, but the philosophy that sprang from this was one of recurring cycles, not one of progressive evolution. Constant change was pictured as ending periodically in a renovating cataclysm, after which came a new creation, with a surface of land and sea, plants, animals, and men, starting where they had started in previous cycles. Degeneration in the meantime was not emphasized except in the morals of men. But progressive evolution was not in the picture.

It is not easy to see why an idea which should have been so congenial to the Greek mind failed to appear. One can only surmise that the lack of abundant fossils was an important factor and that, had animal evolution been worked out, a consistent history of physical changes would also have appeared. One cannot help wondering what might have happened if Anaximander in the sixth century B. C. had seen the collection of Italian fossils, exhibited 2000 years later in the Vatican under Pope Sixtus V, all carefully described and figured, and all explained as due to the influence of the stars on dead matter.

ARABIAN SCIENCE

In the eighth century of our era the Caliph of Bagdad acquired from Christian sources copies of the extant works of Aristotle. The prices exacted by their Christian owners are said to have been far out of pro-

[1] F. D. Adams: *Origin of springs and rivers—an historical review*, Science, n. s., vol. 47 (1928) p. 500.

portion to their value, if judged by their previous use. These and other Greek writings were translated into Arabic, and a succession of caliphs became patrons of science. For more than 500 years the scientific progeny of Aristotle was in Arabia. It was late in the thirteenth century that Albertus Magnus, teacher of Thomas Aquinas, reintroduced Aristotelian science into Europe by translating Arabic writings into Latin.

An outstanding scientist in Arabia was Avicenna in the tenth century. At the summit of his geologic thought was his classification of mountains into what a modern physiographer would distinguish as (1) those upheaved by the constructional forces of diastrophism, and (2) those that are carved out by the destructional process of selective erosion. Here was a physiographic principle of the first order. This idea of differential erosion came into Europe through Albertus Magnus, but it appeared only sporadically before the time of James Hutton late in the eighteenth century. Even in the nineteenth century, and even in America, the extent of its application was not realized until men began to think in terms of peneplains and cycles. Of course it is not implied that Avicenna grasped the full importance of his own generalization.

BEGINNINGS OF MODERN GEOLOGY

From the death of Strabo, Seneca, and the elder Pliny, all in the first Christian century, to the opening of the sixteenth century, nothing need be said about geology in Europe. At about the time when Columbus was exploring America, Leonardo da Vinci was digging canals in the basin of the Po River, incidentally collecting many fossils, explaining their origin, distinguishing marine from fresh-water forms, asserting the former extent of the Adriatic Sea, and describing the encroachment of the Po delta. Perhaps if Leonardo had not been so illustrious in other lines he might now be known as the first Christian geologist. Other noteworthy excavations and fossil collections followed, and other men stated clearly the relation of fossils to sediments.

Had similar circumstances attended a similar scientific start in the time of Pericles, it is hard to doubt that a single century would have sufficed to work out a geologic history whose main outlines would have been similar to those of Hutton and Lyell. What actually followed Leonardo da Vinci was 3 centuries of wearisome disputation, first as to whether fossils are indeed the remains of former living things, and, if so, could they all be explained as left on the land by Noah's flood. Other explanations of fossils showed less vitality than the Scriptural Deluge, but some of them died hard. Among these were the plastic force of Theophrastus, Aristotle's star pupil, the sports of nature, the influence

of stars and planets and of exhalation from the earth, the notion that God made fossils to test and confirm the faithful, and the opposite notion that the Devil made them. To point out this or that man who saw the truth is like counting morning stars. They are easily seen and beautiful to behold, but their beams do nothing to lighten the prevailing darkness.

On the relations of sea and land—diastrophism in general—truth appeared and disappeared very much as on the meaning of fossils. That the best thought on this subject in the ancient and Arab worlds made little impression on Europe in the centuries preceding the American Revolution may be judged from the writings of Leibnitz at the opening, and Buffon at the middle, of the eighteenth century. Both of those eminent scholars entertained the view, discarded and disproved by Strabo, that the primitive ocean overspread the mountains and partly disappeared later by entering those very convenient caverns which have served so many purposes both to Greek mythology and to a quasi-Mosaic geology.

Among the services performed by these mythical caverns in the seventeenth and eighteenth centuries was the explanation of the tilting of beds by the collapse of cavern roofs. The idea of lateral compression was slow in coming.

In an introduction to the rise of physiography, the conception of a wasting surface is fundamental. Like the true nature of fossils and the vertical movements of the earth's crust, true conceptions of erosion crop out here and there, but without systematic advance. It would seem that the followers of Pythagoras had all the basic concepts necessary to understand peneplanation; and that Arabs of the tenth century were ready for an explanation of mountains in terms of first and second cycles. It is true that John Ray at the dawn of the eighteenth century in England was much concerned over the rapid destruction of the continents by rain and running water; that Buffon in France at the middle of that century dared to contemplate the base leveling of mountains (though not using that word); and Generelli at the same time in Italy concluded that the vast and rapid wastage of mountains and continents must be balanced by constant or occasional upheaval. Yet none of these men were free to follow the lead of their own logic. At all events there was nothing cumulative. Apparently what each man believed was his own affair, to be settled with the Church rather than with other students of Nature.

Most geological theories between the first and the nineteenth centuries were cramped and more or less vitiated by the small allowance of time since the creation of the world 4004 years before Christ. This was one of the chief reasons for the emphasis on catastrophes or cataclysms, such as Noah's flood. By repeatedly agitating the ocean and then allow-

ing the suspended matter to settle, as Leibnitz did, it might be possible to make in 1000 years all the sedimentary beds that actually required 1000 million years. There were many similar devices both before and after Buffon; at about the time of our Declaration of Independence originated the idea that the 7 days of Creation were figurative and that the real span of time was, by his reckoning, 132,000 years. It will readily be seen that all limitations of the time allowance and all dependence on cataclysm were hostile to any true conception of the work of erosion and sedimentation.

The understanding of the work of rain and running water may be considered in three stages, or in four if we count the primitive conception of "everlasting hills" which even yet dominates the thinking of many people outside of the Geological Society of America. Three advances followed. There appeared first the mere universal fact of degradation as known to some men in the ancient world and others down to the late eighteenth century. Then came the daring proposition that streams make their own valleys. It sounds very simple, but it means that topography is, in the main, carved out and not built up. Some Greeks, Romans, and Arabs saw this, and James Hutton, who died in 1797, saw it clearly. His friend and interpreter, John Playfair, expressed it in language which has never been excelled, but which was a little too sweeping for Lyell, or for us. Even in the geological world (so far as there was one) this principle did not cease to be debated before the time of our Civil War. It barely had time to take its place among the fundamental data of the science when the third stage arrived, in which moving water does not act aimlessly, carving valleys at haphazard, and leaving hills distributed fortuitously, but works to a pattern whose specifications are as distinctive as the sutures of a nautilus or the venation of a leaf. This is the stage of modern physiography or geomorphology.

MODERN PHYSIOGRAPHY

Up to the last stage just mentioned, all of the observations hitherto described, while mentioned for their physiographic bearing, relate to principles that underlie all geology and precede specialization. But geomorphology is a specialized science—the study of topography from a genetic standpoint. It is not independent, for it interlocks intricately with other branches of geology, but its direct objective is the understanding of topography considered on its own merits, regardless of its services to structural or historical geology, stratigraphy, or geography. Of course the usual experience of scientific study is repeated—namely, that the further a specialty is pursued the more it is found to be interlocked with other branches of scientific study and the more helpful does it become to

them. Yet the basic fact must not be overlooked—that the inspiration and the goal of physiography are to be found in the morphology of the surface, treated in its own right. This is so modern that the lives, if not the memories, of some of the surviving Fellows of this society cover its history.

It was stated that erosion works toward a pattern. But the pattern is not always the same. Moreover, during the process of its development and later during its destruction, various stages are passed through, each having its distinctive style of topography. In an oversimplified formula one might say that the number of possible types of erosion topography is equal to the number of ideal patterns multiplied by the number of distinctive stages in the life history of each. Any geologist will see that this statement is grossly generalized for the sake of brevity and emphasis. But the principle that it does emphasize has become the basis of the classification of topographies.

Modern systematic physiography is not the product of any one nation, much less the work of any one mind. But, among nations, the United States has been much the most active and productive; among men, William Morris Davis has been far and away the leader. Only among people who do not know how science grows might it be necessary to state expressly that he did not build on nothing. Had Davis never lived, the combined work of Powell, Gilbert, McGee, and others would have left a general picture of land masses being reduced to base level by streams of various origins, life histories, and characteristic courses, the forms of unconsumed masses being determined by structure, climate, and other factors, even including age. But this is only a rough frame and largely empty, and the lives of these men were spent largely in other phases of geology, and it was left to Davis to paint the panorama of changes from initial forms to their final disappearance. This is the theme of the geomorphic cycle, the most distinctive element of modern physiography.

The contributions of Europe are by no means to be ignored even though, for the most part, they come within the period dominated by Davis. The work of Baron Ferdinand von Richthofen may be in a rough way classed with that of Powell and Gilbert. I say "in a rough way," because Americans are justly cautious about allowing anyone to be classed with Gilbert. Albrecht Penck's *Handbuch für Morphologie der Erdoberfläche* (1894) is morphographic rather than morphologic. Penck's own contributions to genetic physiography are mainly of later date. A definite and distinctive contribution from Germany to the geomorphology of arid regions was made by the work of Siegfried Passarge early in the current century. If the illustrations used in this brief account of the science are taken

mainly from the work of rain and running water it is only because that field is much the largest and most important. The mention of Passarge is a reminder that wind is an effective agent in another cycle of change. Waves and currents of the sea and lakes develop shore topographies, also in systematic order.

An American writing thus to Americans may incur the accusation of assuming an unwarranted degree of finality for the system of which Davis is the chief exponent. Most objections need no mention in so brief a sketch because they relate to minor matters that do not affect the general frame already suggested. Until recently most criticism has centered on two points, so closely related that they are virtually one. One of these is Davis's terminology; the other is his habit of describing a topography in terms of its genesis instead of using noncommittal—*i.e.*, empirical—terms. If these questions are not argued here, it is not because of any disposition to ignore or minimize the risks incurred in using a genetic nomenclature, but rather because neither of the issues raised affects the science fundamentally. Davis's own repeatedly expressed attitude on terminology was unimpeachable—namely, that all terms must run the gauntlet of natural selection and survival of the fittest. The author may *propose*, but the scientific public *disposes*. The same statement applies to methods of technical procedure.

In recent years, mainly since the appearance of Walther Penck's *Die morphologische Analyse*, there has been some debate relating to the part played by crustal movements in the details of topography or, conversely, the extent to which a given topography reveals the details of crustal movement. Much of the argument hinges on the constancy of slopes not controlled by structure. The general effect of Penck's contention, if established, would be to magnify the role of diastrophism by ascribing larger survival value to its immediate results.

As the science now stands, with or without the modification just suggested, the topography of an area is characterized and classified by answering three questions: (1) What was the structure on which erosion had to work? (2) What was the agent or process? and (3) What stage has been reached in the cycle? This formula—*structure, process* and *stage* (sacrificing exactness to brevity and emphasis)—has more than once been elaborated by Davis before this society. It sounds simple, but its application is beset with complications. For one thing, all significant events of diastrophic history must be comprehended under structure. It is enough to say here that, consciously or unconsciously, with or without Davis's nomenclature, something like this formula is being used throughout the geologic world in the study of topographies made by any kind of erosion.

PHYSIOGRAPHY AND GEOGRAPHY

Like any other branch of geologic science, physiography may be pure or applied. As pure science, it has its roots in the other branches of geology as well as in physics and climatology. With reference to these, physiography is, in a very true sense, the end of a story. As applied science, it is commonly assumed that its main function is to afford a sound foundation for geography. It matters little whether the geography be physical, political, historical, or economic. Here it comes at the beginning of the story. In this broad view, physiography is, in the main, produced by geologists and used by geographers.

As the expression of an ideal this is no doubt true; so true that it might, not unnaturally, be expected that every geographer would be familiar with physiography and would be conscious of its data as constant factors in all human geography. The degree to which this ideal has been realized in our own country is not very high. The streams of earth science and human geography seem thus far to have flowed in parallel courses without much mingling of their waters. It is not often that any physical concept appears in the literature of human geography that might not be expressed by the terms high and low, rough and smooth, wet and dry, fertile and barren, or their like. The niceties of classification of topographies are very little drawn upon. Lest this statement sound harsh it should be remarked parenthetically that geography has made substantial use of climatology, which is quite as basic as geomorphology.

Whether this situation is, or is not, desirable does not come within our theme. We are speaking only of the status of physiography among other branches, and it appears that its significance thus far has been largely in the give-and-take within the family circle of geologic interests. No doubt one of the reasons why physiography has flourished in the United States is the lack of separation between geology and topography in our official surveys. Physiography and other studies that serve geographic interests have been nursed by geology, as ethnography was in the days of Powell and Holmes.

As late as one generation ago in our country, geography was getting its start in higher education, largely under the wing of geology. This tended to favor geography as a real science, giving it a rational basis and preventing it from becoming mere regional economics. In many if not most institutions of higher learning in America, geology and geography are still taught in a single department, and students go freely from one science to the other. Nations differ in this respect. In Germany the two subjects may be in separate departments (institutes) but within the same faculty, that of Philosophy. In France the faculty of science is quite distinct from the faculty of letters, and the separation is strict. Geog-

raphy is in the Faculty of Letters with history, and the passion of the French for system has resulted in assigning physiography to geography, with the commendable purpose of making sure that future geographers will be well-grounded in the underlying science. However, what it did actually make sure of (in most cases) is that physiography will not be taught by physiographers but by men who know it only at second hand. For it will be remembered that physiography is *produced* only by geologists. There is of course the exceptional case not contemplated in the general plan, in which men like Emmanuel de Martonne have deliberately qualified in one line and then gone back to qualify in another. Most men think that the long steep road to a French doctorate is long enough without retracing. The large fact remains that the system prevents the creative physiographer from teaching his subject and thus begetting scientific progeny.

This is, of course, not an educational paper, but the future of the science cannot be ignored. The danger of drying up the springs of physiography both in the university and in official surveys is worth considering. It is by no means sure that this tendency does not exist in Europe, including England. Certainly the recognition accorded to physiography by two such powerful scientific organizations as the United States Geological Survey and the Geological Society of America cannot be matched anywhere else in the world.

PHYSIOGRAPHY AND STRUCTURAL GEOLOGY

The status of this branch in the geologic family will be determined mainly by the services it can render. As a single illustration, consider its relation to structural geology. To whatever extent surface forms are determined by underlying structures, to the same extent structure is revealed by topographic forms. Now, topography is the first thing in evidence. The structure may be almost equally evident (if seen by the right man) or it may require weeks of patient plodding. Both surface forms and structural forms may be too large to be comprehended in a single view. Not until they have been laboriously measured, plotted, and correlated, does the mind's eye see them in their true relations.

These statements were literally true a few years ago. They are less so now, since both topography and structure are beginning to be studied from the air. The form of the surface is of course the main thing seen, often the only thing. But the degree to which that form reflects structures is one of the surprises among recent developments. Lines and forms as seen from the ground may be so obscure as to evade the eye of the geologist even after being seen clearly from above. Some commercial companies are now doing combined physiographic and structural work by airplane.

This is merely applied physiography. That its use will increase greatly can scarcely be doubted, especially in reconnaissance of the vast wilds of South America and other little-known regions where surveys on the ground would be prodigiously expensive and probably futile. Aside from good engineering and photography, the success of work from the air will depend mainly on the proper interpretation of topographic forms. All knowledge of systematic physiography will be subject to requisition. Some of the structures detected will be too obvious to require training or experience—like the "sheep herder anticlines" of the Wyoming oil fields. But from these at one extreme, in which the relation between form and structure is simple and direct, there are all gradations to the other extreme, in which the relation is indirect and complicated and can be seen only by the specialist. The field for such study promises to be important, and the work thus done will go a long way to justify the elaborate deductive studies and the multitudinous drawings made by Davis and others, depicting the successive forms that should appear in the landscape in a long history of erosion, perhaps complicated by intermittent diastrophism or aggradation.

CONCLUSION

Probably the present outlines of the science will prove to be fairly permanent. The most significant alterations may come by way of quantitative studies. Naturally these cannot be pointed out in advance, but new principles may emerge unexpectedly when the effect of this or that process can be measured and balanced against others. This is especially true in deciphering the history of a given area. We risk the assertion that the science will be put forward faster by directing effort primarily to the survey, interpretation, and description of the earth's surface in the light of current theory than by giving primary attention to revising the system. Perhaps this statement is meaningless, since both must go on together. But it at least expresses our judgment, and the fact that the science of genetic physiography is more or less grown up, and that its place in the world will be determined largely by what it shows itself able to do.

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THE PROBLEM OF PETROLEUM

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(*Anniversary Day Address*)

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INTRODUCTION

The subject of this discourse is not my own. It was selected for my friend William E. Wrather as suitable for a paper which he contemplated writing for the present occasion. Unfortunately, Mr. Wrather's health does not permit him to be here today; we trust he may soon be back at similar meetings. The present paper, written and presented in consequence of an invitation by the Secretary, is my personal elaboration of the petroleum problem and a few suggestions toward its solution.

The semicentennial celebration of the organization of the Geological Society of America furnishes an auspicious time at which to appraise the origin and progress of petroleum geology and briefly to discuss its problem; for the profession was born and matured during these scientifically momentous 50 years. Our Society has discussed petroleum problems in many of its meetings.

In the present attempt to search history and economics and review the petroleum problem it is possible that some cherished traditions may be relinquished. I do not propose to duplicate past accounts of the history and accomplishments of petroleum geology, however excellent these have been, but will launch upon a sea of originality.

THE FIRST PETROLEUM GEOLOGIST

We may first ask, Who was the first petroleum geologist? Neither you nor I can be identified with this individual; neither is he to be found among our fathers, uncles, or contemporary teachers. Nor, with all due respect, was he one of those able workers who delved intelligently into structural and stratigraphic problems from the year 1850 onward.

The first investigator of oil problems lived much earlier than that. In fact, his name is not found in American or European History, but possibly in Ancient or pre-History. This forgotten man may have been some Chinese engineer who, millenniums ago, searched for the relationship between a certain objectionable grease associated with salt water and the valuable gas for which he was drilling by means of crude bamboo rods. The said geologist may have been some Babylonian navigator, looking for asphalt seepages with which to caulk his peculiar round boats, such as are still seen on the Rivers Tigris and Euphrates. He may have been a general of Alexander the Great, who required "naphtha" with which to fill the tanks in artificial horses, constructed for incendiary purposes during an Asiatic siege. Or, the unknown geologist may conceivably have been Lot, the nephew of Abraham, driven from blazing Sodom by a sudden slipping of the earth's crust in the great Dead Sea graben and the simultaneous emergence from the fault plane of confined gas and oil, now known in the form of seepages.

DEVELOPMENT OF PETROLEUM GEOLOGY

Today we have time only to skim the history of oil geology, but we must mention factors which have contributed to the solution of petroleum problems. At the same time we must ask ourselves, What is the *problem* of petroleum today? This commodity, used by the ancients for mortaring, military incendiarism, embalming, and for medicine, and by the present generation for fuel, lubrication, and purposes too numerous to mention, has produced frequent changes in theory of its source, origin, migration, and accumulation.

I do not propose to start a discussion on any of these subjects. To the ancients petroleum may indeed have been "where you find it"—mixed with salt water on the surface of Chinese wells, polluting the earth adjoining fire temples of the Apscheron Peninsula, bobbing up in the form of bitumen cakes on the waters of Lake Asphaltites, and never raising serious questions of quality, source, origin, structure, hydrostatics, metamorphism, sand-thickness, or porosity.

No important scientific question on the subject can have been raised until the latter part of the nineteenth century, when the "anticlinal theory" was gradually evolved through observations by Henry D. Rogers,

I. C. White, Edward Orton, E. B. Andrews, F. W. Minshall, Hans Höfer, William A. Earseman, and others. For a time the words "anticlinal theory" constituted a magic phrase, and geologists hastened to avail themselves of knowledge which might conceivably lead them instantly to new discoveries. For decades this theory quietly smouldered in oil circles, prevented from breeding an army of petroleum geologists only because of limited demand for oil products and the inherent conservatism of a majority of operators, few of whom had at that time faith that oil geology was more than a passing fad.

And, as we now realize, not all petroleum geologists themselves were satisfied with the theory. Its merits and seeming exceptions were debated often on platforms of geologic gatherings. Although the anticlinal theory was perhaps the greatest single contribution to scientific location of petroleum and natural gas, it left many problems unsolved. When our generation of thinkers came along some one was progressive enough to contend that not all petroleum is found on anticlines. The ensuing heresy caused emergence of the "structural theory," by virtue of which an attempt was made to assign all known discoveries to some place in a small group of structural types.

The structural theory has survived many vicissitudes. Domes of several types, terraces, noses, or plunging anticlines, dry synclines, unconformities, crevices, lenses, "shoe-strings," and shore lines all have had due publicity and have served to facilitate discovery of oil in many states and countries. Faults, which were once anathema to oil geologists, found their place in professional research.

From the early concentration of attention to simple folds, the studies proceeded to all phases of normal and asymmetrical anticlines, overthrust folds, and lateral changes in sedimentation. Oil was found in ancient coral reefs. Thickness of geologic sections became a factor. The relative ages of folding were given greater weight. English geologists working in Iran discovered intraformational flowage of salt, gypsum, anhydrite, and clay, rendering surface structure useless in certain areas. Knowledge of salt domes brought to light exaggerated examples of flowage in the coastal structures of Texas, Louisiana, Mexico, Germany, and Iran. For several decades a small group of geologists was scouring the earth in search of oil, yet without present-day illumination or adequate knowledge.

The United States Geological Survey and similar organizations have done valuable work, but their efforts have been limited necessarily to strictly geologic endeavors. Members of the Geological Society of America, the American Association of Petroleum Geologists, and the American Institute of Mining and Metallurgical Engineers have probably done more than is generally done for the enlightenment of colleagues in any pro-

fession. Oil companies have in general proved liberal in allowing cooperation by their personnel and the publication of results. But, about the first decade of the twentieth century, a definite contribution to methods was needed.

Then came a little-known Government engineer—William T. Griswold—who, together with M. J. Munn, declared that old methods of studying the attitude or "lay" of oil-bearing strata were antiquated. He prevailed upon the Director of the Survey to allot funds with which to run lines of levels to oil and gas wells and "dry-holes" (the last mentioned constituted the group authorized most reluctantly). The earliest studies of this sort were made in western Pennsylvania, Ohio, and West Virginia.

Well logs had already been studied in preparing reports for government surveys and oil companies; Griswold sifted the accurate from the inaccurate, proving that grievous errors of various kinds had crept into the determinations and into all mapping done on the basis of old-fashioned cable-tool logs. Core drilling was introduced for mapping and sampling. A few geologists insisted that samples be taken from interesting wells. Other workers commenced similar studies in northeastern Oklahoma, and subsurface geology thereby became of age. Complexity of the facies in formations encountered soon led to adoption of the sciences of micropaleontology and studies of heavy minerals in connection with drilling.

About that time we were wont to hear from still skeptical laymen that "Geology is at any rate useful in showing where *not* to drill." This was indeed one of the most valuable functions of petroleum geology in its younger days, when wildcat wells were usually sunk owing to a speculative urge by men who could not be expected to know that a well drilled in granite is almost certainly dry and that a metamorphic complex is not a logical area in which to drill.

Another step in advance was the "carbon-ratio theory" of Dr. David White, meeting with some opposition at first. Came its second breath and the realization, on the part of geologists, that this theory furnished one means of determining whether a region is too highly metamorphosed to carry commercial oil deposits. The application of the "carbon-ratio theory" resulted in the drawing of "isocarb" lines to show lateral changes in the amounts of fixed carbon in certain coal beds. Came, thereafter, many ideas which helped in one way or another toward delimitation of the world's oil fields prior to drilling. Source rocks were studied, oil field waters analyzed, and caprocks given attention.

TECHNICAL PROGRESS

However, technical progress did not attain its full momentum because of any theory. In early days comparatively little was expected of a field

geologist; his clientele was generally satisfied with the discovery of an anticline, although sometimes, if the field was a failure, suspicion arose that the structure upon which it was located might not be an anticline after all. Gradually geologists were forced by increasing demand, as well as by professional competition, to be more thorough, more decisive, and to furnish more detailed information. Structural studies in particular required more of a geologist's time. The nineteenth century custom of expressing a mere opinion graded into that of the twentieth century in which a map was demanded. At first this was roughly sketched and the structure of the surface formations plotted by means of a pocket compass, pacing, and barometric determinations—a method still useful in primary reconnaissances, especially abroad.

The work of the government in classifying public lands brought plane table and alidade into use for mapping topography and obvious surface structures with fair rapidity. Although contour lines had been used in earlier government publications for showing regional structure, they now came into vogue for accurately delineating anticlines, synclines, and domes. From structure-contouring of the surface it was only a step to contouring deeply buried structures of somewhat different configuration. "Convergence maps" began to be used to assist in contouring the subsurface by means of what were at first called "isochore lines," and later "isopachs."

Aided by the structural classification and by the adoption of plane-table methods of "detailing" (as survey work was now called) petroleum geology was outward bound on its voyage of success. Although E. B. Andrews had been the consulting geologist of an oil company as early as 1864, few men practiced this profession for half a century. Those decades were marked by an increasing need for geologic work, since the percentage of dry holes ranged from 17 to 36 according to the part of the United States concerned. Commencing with two small consulting offices in 1908, at which time the author of the present discussion commenced his professional career, the number of practicing petroleum geologists in the United States slowly increased. In the beginning no great demand for their work was evident, but at the end of the first decade of the present century the volume of business became too great to be handled by a few consultants, and the number annually increased many fold. Field parties, generally consisting of two or three men each, made their appearance in all producing and near-producing States and sometimes almost trod on each other's toes. It became fashionable for oil operators to have their properties "detailed" in advance of drilling.

This last was only one function of a petroleum geologist's work, for he was thereafter expected to report on water and casing problems, com-

position, texture and porosity of oil-bearing beds, rate and nature of water encroachment, chemical characteristics of the water and associated gas and oil, the relations and effect of gas pressure, deviation of wells from the vertical, value of the properties surveyed, extent of reserves, and so on. A geologist's duties graded into those of the new profession of "petroleum engineer."

THE ROLE OF GEOPHYSICS

Up to that time the instruments used had been few in number. We cannot here follow all of the advances in technique during ensuing years. A reactionary economic cycle and the World War brought the country to a condition in which adequate oil supplies were not "in sight"; alarmists (and let us remember the beams in our own eyes!) believed the end of the American oil supply was approaching. A new momentum of petroleum geology ensued, more modern technique was used, and new methods evolved in search of fields.

In central Europe a gravity pendulum made its appearance to map subsurfaces in the plains of Hungary. Then, with little immediate attention from American geologists, the torsion balance was designed by Oëtvös and manufactured by the establishment of F. Süss.

The same apparent shortage which caused the first commercial use of torsion balances witnessed evolution of the seismic method of mapping subsurface structures. Sidney Powers had published papers showing that deep-lying strata are not generally roughly parallel to those on the surface as had been assumed up to that time, but that "buried hills" might be found even in the midst of synclines. The principal purpose of geophysical instruments was at first to outline the position of such features and of hidden salt domes. The theory of isostasy was used in gravimetric determinations.

From that time to the present the geophysical methods of geologic mapping have been systematically perfected. Gravimetric, seismic, magnetic, electrical, and other methods have been freely used by oil companies. At the same time, radioactivity, thermal action, and chemistry have facilitated structural delineations and the choice of drilling sites. Isogeotherms and isosalinity lines came into use on some maps. All petroleum geologists now appreciate the helpfulness of geophysics, and they should be grateful for the discovery of its methods.

Nevertheless, a note of warning must be sounded which might have saved millions of dollars if it had been radioed over the world when geophysics first came into use. Far from being an infallible means of oil discovery when used alone, the new science is useful only when its method and practice are controlled by a supervising geologist. Like the barometer, plane table, microscope, and airplane, geophysics is *one* of the tools of

our profession, but geophysics is not an isolation science nor a purely physical one. The physicist has his place in oil geology just as has the chemist, petrologist, and paleontologist; and modern oil geology owes him much. The geophysicist does not do his work for the sake of the mathematics involved but is called into consultation when required on a specific geologic or engineering problem. The interpretation of results, and the time, place, and methods adopted are, however, the oil geologist's sphere. It is an expensive error to suppose that a geophysical department of a company or of a university has a place except as an adjunct to a geological department; for, in earth determinations, physics and geology must proceed hand in hand and be geologically controlled.

The world's technique of finding oil reserves has been limited mainly to the 50 years' existence of our Society. When, in 1916, the profession required a Society of its own, the American Association of Petroleum Geologists (at first known as the Southwestern Association of Petroleum Geologists) was founded. Perhaps, if a curve could be drawn to show the tangible results of geology, we might find the line of successes rising irregularly but with annually increasing rapidity toward an unknown peak. And there can be little doubt that discoveries of new methods of geological and geophysical work and of drilling will continue to be made.

Therefore, having rapidly surveyed the field of geologic technique, and being fully aware of the existence of imperfections in present methods and interpretations, the author of this discussion feels, nevertheless, that the Society has every reason to be proud of the achievements of oil geology. It is clear, then, that the "Problem of Petroleum" is *not* one of perfecting its technique. The latter will doubtless continue to be improved from time to time by companies and individuals; but the search for ultimate perfection no longer constitutes our underlying problem.

THE QUESTION OF ORIGIN

Soon after the discovery of the Pennsylvania oil fields controversies arose regarding the origin of petroleum. During subsequent decades the proponents of the "inorganic theory" have had a hearing as well as those who advocate organic theories. The question of origin has ceased to be an important matter to oil operators, if it ever was one. They find oil if they search for it scientifically with adequate capital; nevertheless, a discussion of the mode of origin continues in scientific circles.

Early in the history of American oil, the most widely accepted theory of origin was based on the decomposition of carbonaceous material; some geologists have since tried to prove that petroleum was generated solely from vegetable matter, and at least one well-known theorist still argues

for a relationship between coal and oil. Most of our members probably now accept an origin from decomposing organic matter, of which the animal content is believed to be most important; but, in any case, decomposition apparently took place under definite marine conditions. Some of our members are now working on this question. But, whatever the origin of petroleum may be, it is clear that this does not constitute the fundamentally important problem for which we are inquiring. We must look farther afield.

INTERRELATIONSHIP BETWEEN PRODUCTION AND RESERVES

What, then, is "the problem of petroleum"?

It must be something less tangible than mere discovery technique. If any discussion of this problem be useful we must take into account the mental perspectives through which professional progress advanced from surface reconnaissance to aerial surveying and subsurface mapping, from a belief of a large portion of the public in divining rods to the universal acceptance of geophysics. But progress is not an even curve; advance has been held back at times by lagging technique, at others by overproduction due to slackening demand. Overproduction causes lowering of prices, and lowering of prices causes industrial apathy.

The average rate of technical improvement may be satisfactory, as has been said, but the consistent problem of oil producers is how to produce and distribute the volume of petroleum which is economically necessary at any given time. The solution of this technical and economic problem will depend on full coordination between economics and technique.

Volumes have been written about the discovery of oil in Pennsylvania in the year 1859, but petroleum was produced much earlier. Official figures for the Pechelbronn field of Alsace are complete from 1812 up to date, with the possible exception of 1817, 1822-1824, and 1826. Refining by primitive methods was tried as early as 1627, and the archives of Pechelbronn disclose a drawing, completed in 1768, of an invention designed to supply "petroleum oil burning in lamps without leaving any nasty smell." The Russian fields produced during a similar period as those of France, and experiments in oil consumption were conducted at Baku prior to 1723.

In the year 1796 Alsatian petroleum was used for lubricating wagon wheels; 1813 marked the sinking of 142 wells attaining depths of about 640 feet. Many foreign countries attest to correspondingly early development of petroleum in primitive ways, and American figures tell us that oil was set on fire in 1740 in a ceremony of the Indians of western Pennsylvania. "Seneca oil," derived from the surface of Oil Creek, was sold for medicinal purposes, and in 1833 Professor B. Silliman, Sr., investigated

a large oil spring in southwestern New York, the report on which was published with a consideration of the distribution, character, and probable origin of petroleum. Experiments in manufacturing oil from coal were tried in Scotland, France, and the United States. The date of the famous Drake well of Titusville, Pennsylvania, having an initial production of 25 barrels per day from a depth of 69½ feet, is widely referred to as the oil industry's first birthday. Ensuing periods of "feast or famine" on the part of oil producers were due to alternations in over- and underproduction such as have always beset the industry. Thereupon hangs the history of the ensuing fluctuation in value of crude oil between 10 cents and 20 dollars per barrel.

CYCLICAL ASPECTS OF THE PROBLEM

At this moment perhaps no oil geologist envisions a petroleum shortage. Reserves concededly in sight, which in 1925 were only 5½ billion barrels, now aggregate 15½ billion. They have become the greatest in the history of the industry in a period when industrial, geologic, and geophysical technique may be highest. Improvements in refining methods have come rapidly, owing to which increasing proportions of products are extracted from crude oil, so that some persons might expect advance to continue indefinitely at a similar rate. Even with a large increase in consumption, during a business boom or a war, we have oil in sight for a considerable period.

Yet, this valuable substance *is* exhaustible, as every geologist knows, and technical improvements are certain to slow down sooner or later because of the very intensity of recent developments if for no other reason. One of our States after another has been tested for oil, until 22 of them now produce; the yield of some has already been stabilized. The maximum drilling depth, which in 1895 was 1200 feet, now registers over 15,000, and the rate of increase in maximum depth is annually increasing. New oil deposits continue to be discovered, even below 10,000 feet—a depth once thought prohibitive to oil occurrence—and their ultimate attainment may be astonishing. How far the drill can continue to go none of us can say.

But natural limitations exist in the positions of bottom and flanks of any sedimentary basin. There are only a certain number of possible oil-bearing States, as is likewise the case with foreign countries. Consequently, even if technique should advance to an unlimited extent, new discoveries in any productive State will in time cease, and only the producing fields be available for normal supplies. Even if refining and consuming technique now result in added saving of oil products, these will be more than offset in the long run by new uses and multiplied mechanization.

Even if, by any chance, the United States should stop increasing its consumption of oil products and the number of their uses, certain foreign countries will certainly continue to demand increasingly greater volumes of oil—particularly the Asiatic countries, some of which have hardly begun to expand commercially.

We have noted that the fundamental problem of petroleum is not at the moment geologic. The fundamental questions involved are pertinent to either an over- or undersupply. The existence of vast unsuspected reserves in some countries is proved by foreign researches. A great number of geologists are now at work abroad, and the use of geophysical methods in certain countries will result in finding more oil than is now "in sight." Yet all this constitutes a passing phase; a reaction is sure to come simultaneously with any great emergency which demands many times as much oil as is now being found. The oil world must be ready for that day.

It is necessary to emphasize the probability that we may be faced at some time with a termal shortage within a few months after floundering in an apparent surplus. If we consider the relatively great reserves known when the Cushing field in Oklahoma was discovered in 1912, we may recall that an actual shortage emerged in less than 5 years. We may also note that Soviet Russia, which, a few years ago, was making every effort to sell surplus oil throughout the world, has recently been obliged to suspend its exports owing to increased internal demand and decline of its principal field. We have recently seen the Mexican oil industry ruined owing to the mooted suicidal action of that government itself. Most of us recall a sudden reversal which took place in "Golden Lane" activities nearly 20 years ago when the influx of edge-water was first announced there.

A great war would reverse the relations between production and consumption. World consumption has increased about 30 per cent in the past 4 years; Asia, with 55 per cent of the world's population, has less than 5 per cent of oil consumption but is rapidly expanding uses. The United States has to date produced 64 per cent of all oil consumed in the world. These factors indicate the increasing need for petroleum products abroad and the necessity for continued diligence on the part of American operators in discovering and developing foreign fields in particular.

THE MAIN PETROLEUM PROBLEM

Undeniably there are *many* problems of petroleum. It would now be idle to attempt to outline them even briefly; every geologist knows that they are multifold; they increase in number annually, and one problem leads to another. The principal duty of a modern petroleum specialist is to solve problems rather than to limit himself to mapping. There are geologic, economic, industrial, chemical, political, and legal problems;

geologic problems alone are of varied classification—genetic, structural, stratigraphic, hydrostatic, volumetric, dynamic, and so on.

But more important than all others is the single problem of how to continue uniformly a conservatively calculated progressive rate of discovery and production and to distribute petroleum where needed by changing conditions. This problem brings us back to the age-old question of supply and demand, as is the case with most of the world's real problems. An ideal situation would be one in which the world would always have sufficient oil "in sight" and never an oversupply. No private initiative nor governmental agency can yet claim to maintain at all times a sane average or conform exactly to changing requirements in this respect for a long period, although carefully elaborated estimates made by the United States Bureau of Mines and by the American Petroleum Institute are highly helpful predictions based on all indices available for a short term ahead.

The present brief review of the history of petroleum discovery indicates that depressions in the cycles of dearth and plenty are not due mainly to technical failings, nor are the cyclical peaks commonly due to "technocracy." Discoveries in the oil fields generally proceed without interruption, and it is now possible to regulate and conserve the development of a field by agreement between operators with State approval. Yet, during times of apparent overproduction, as is the case with inventories of any commodity, the natural tendency is to slacken efforts because of lowering of prices received. This was the case during the World War, when it actually seemed as if American oil reserves might be on the wane. The ensuing boom period again found producers unprepared. The speeding up of all technique at that time produced a momentum in development, leading to the change from a depression cycle to one of apparent oversupply, and this last has since continued in varying degree.

Changing economic cycles also affect geologic education and are thereby important to another group of our membership. Nobody questions the view that increased perfection in subsurface methods, rise in the use of geophysical instruments, and promulgation of artificial inducements to oil exploration were needed at various periods in the past. It was creditable for universities to educate an increasing number of geologists. But it is unfortunate for young geology graduates if the brakes cannot be applied at the proper moment and the numbers of these men limited to those needed to maintain necessary oil reserves. This overproduction of geologists became at times a major factor in causing petroleum overproduction. The new kind of vicious circle caused waste, tending toward early decline of the then existing fields. Many geologists found it difficult to obtain positions; consequently, geologic talent was wasted as well as oil. There seems never to have been a known relationship between number of

graduates and the necessary additions to oil reserves; the plotting of a curve to this end by one of our economists might be constructive.

NEED FOR SCIENTIFIC FORESIGHT

In a scientifically fascist State it is probable that an arbitrary allotment would be made of the number of young men who must study paleontology, seismology, and so forth, based on the outlook for oil reserves and number of individuals working in stated specialties. Although such an edict might settle *some* difficulties, let us hope that Americans will refuse to consider any such government regimentation.

Nevertheless, since we always have too much oil or too little, it behooves us to look ahead. Chemists tell us that the manufacture of substitutes is feasible in any one of several directions—hydrogenation from coal, shale, or vegetable matter, polymerization of gases, and so on. Oil has successfully been made from coal in Germany and in Great Britain, but at prohibitive cost; such methods will not be needed until a great emergency causes an unexpected shortage not replaceable by drilling.

The role of The Geological Society of America is, of course, purely scientific. But the industry records an imposing array of technical advances in removing objectionable sulphides, increasing the manufacture of petroleum by-products, prevention of losses, improved methods of recovery, prolongation of the life of wells, hydrogenation, polymerization, installation of safety appliances, wider spacing of wells, water-flooding, repressuring of "sands," and some other advances.

Since the petroleum industry continues to advance technologically, a dangerous shortage may not come from a normally declining trend of production or an upward trend in consumption, but may be due to unexpected conditions, such as a great war. Conversely, the oil business conceivably might be ruined through some unexpected discovery, if this made the use of petroleum unnecessary. Until a serious unforeseen emergency arises the periods of over- and undersupply will alternate with each other and maintain a similar social and economic problem to the present one. A change must come from some other direction than merely preparedness to introduce substitutes on the one hand or to find new uses on the other.

Careful estimates, made by the United States Bureau of Mines and by the American Petroleum Institute, of the amount of oil needed versus that available during any period have annually been a boon to operators who have observed the warnings given. The geologic fraternity studies and acts on these warnings to some extent. It may seem as if universities alone are staunch in their united endeavor to educate men for an unlimited number of positions, which may or may not exist, granting under-

graduates freedom of choice of their careers in entire ignorance of changing economic trends which might affect their whole lives. At times we may have a shortage of available geologists and of certain specialists; at other times the condition is reversed.

Unfortunately, any problem of the oil industry is ignorantly or maliciously made much of by journalists, politicians, and government agencies, with seemingly no general desire to find a solution. This attitude results in outside interference, unjust criticism, unwise regulation, and unnecessarily burdensome taxes. The petroleum industry can work out its problem of the moment, as it has already mastered many of its technological problems, if interference be removed.

What the government cannot consistently do, can, however, be done by universities, which likewise have their share of problems. The petroleum problem, so far as it affects geologists, is not entirely a geologic one. Economics, accounting, the theory and practice of logic, business methods, business law, physics, surveying, and commercial geography may be more important to a geologic graduate than an overbalanced knowledge of paleontology, chemistry, or theoretical geology. At least, students should be required to study enough of general business subjects in their early years to enable them, with the advice and assistance of university faculties, to place themselves on the track of financial as well as scientific existence—this even in opposition to an individual's personal likes and dislikes.

In general, it is probable that specialties should be left for postgraduate attention, yet university courses should be lengthened rather than shortened, and faculties should lay their plans years ahead with the aid of governmental and business statistics. Numerous private business advisory services exist which, for a nominal retainer, will assist in making predictions as to the outlook for graduates of specific courses of study, thereby guiding undergraduates into lines of effort in which the best business opportunities appear. The economic condition of graduates as well as the relationship between petroleum supply and demand may thereby be improved.

In planning for this envisioned rationalization it is necessary to look ahead, not 1 to 5 years, but 10 or 20; for many foreign fields require as much as 5 years for testing and 8 to 10 years to provide them with pipelines, refineries, and ports. No important educational or business program should be considered from a perspective of less than 10 to 15 years. Similarly, foreign explorations should be planned by oil companies at least as far ahead as that.

Of course, not all conditions can be foreseen. Such discoveries as that in East Texas in the early 1930's may sometimes be matters of chance;

yet the proportion of surprise discoveries will lessen as our science becomes quite efficient. The problem of petroleum involves leveling out inequalities between flush and lean production. Technology and exploration can, in their routine processes, handle any probable early decline, but only economic sociology can cope with the upward tendency of technical advancement and increasing education. What petroleum needs most is not perfect technique, but better coordination between the technique of the universities and that of professional men in the industries they serve. If we are to expect ultimate cooperation between the government and industries we must begin by coordinating education and professional practice.

CONCLUSION

Thus, the answer to the question, What is the problem of petroleum?, is answered relatively. During the first two decades of this century the answer was found in the need for perfecting technique; but at the present time, the problem has become one of coordination. If we wish to maintain the petroleum industry as an economically industrial identity free from undue government control we must prove that it can cope with this problem. The last appears to be not alone one for oil executives, but it concerns every geologist in the profession and all educational institutions teaching geology.

Therefore, in closing this discussion, it is necessary to appeal not to undue efforts for improving the technique, although geologists and the petroleum profession will continue to do this in due course with or without their conscious volition. What the petroleum world needs more urgently is better cooperation and coordination between professional men, universities, operators, accredited government officials and all, with a view to evening out inequalities of visible and changing demand, questions of inequable regulation and taxation, changing social conditions, and making our national sources of supply physically, economically, and politically secure for generations to come. Geologists, as well as oil producers, may then go forward without fear either of overproduction or of shortage. Such is an ideal for which every producer and distributor searches but seldom finds. In a fully coordinated science and profession this dream might come true.

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VERTEBRATE PALEONTOLOGY SINCE 1888

BY WILLIAM BERRYMAN SCOTT

(*Anniversary Day Address*)

When the Society began its career, the three famous paleontologists—Leidy, Cope, and Marsh—were living and had several years of activity ahead of them. Wearied of the celebrated Cope-Marsh feud, Leidy had turned aside into other fields of research, and, though Marsh and Cope continued their work as long as they lived, nearly all of the discoveries upon which their great reputation was founded had already been made. A small band of younger men, several of them still living, was making its appearance, and these men gave promise of being worthy successors of the great trio.

A new era in vertebrate paleontology in America opened with the appointment of Henry Fairfield Osborn to the American Museum of Natural History, for this resulted in the development of that museum into the finest and most extensive establishment of its kind in the world. Needless to say, Osborn did not accomplish this great task unaided, but his leadership and the enthusiasm which he aroused appealed to men of great wealth, such as Mr. Jesup, President of the Museum, and Messrs. Pierpont Morgan, Whitney, and Dodge, Trustees, and they supplied him with the large funds needed for the carrying out of his ambitious program. As a first indispensable step Osborn gathered around him a staff of able, vigorous, and gifted men, such as Wortman, Peterson, Matthew, and Gidley, to mention only the dead, who as collectors and investigators were successful, even brilliant, assistants.

When Professor Max Weber, of Amsterdam, published the rewritten and greatly enlarged new edition of his famous work on mammals, *Die Säugetiere*, he dedicated the second volume to Osborn, Matthew, and Gregory, as "den Förden der Säugetierkunde."

Not only has the American Museum been a wonderful center of research and popular education, but its influence has extended over the entire land and has, perhaps, been the greatest single factor in bringing about the multiplication of museums, which is one of the most remarkable and gratifying features of the period under review. In 1888, the only museum in the country that had a great collection of fossil vertebrates was that of Yale University; Cope's collection, while deservedly celebrated, was pri-

vate and was stored in a dwelling house. The Academy of Natural Sciences in Philadelphia had most of Leidy's type specimens, but several of these were in the Smithsonian, at Washington, not yet the home of the United States National Museum. In 1880 Sir Archibald Geikie visited this country and was so much impressed with what Professor Marsh had to show him that he published an article in *Nature*, entitled *Yale College and Palæontology*.

Since that day, a great many university and college museums have followed Yale's example and built up collections, some of them very large, of fossil vertebrates; Harvard, Dartmouth, Amherst, Princeton, Chicago, and many of the State universities including Michigan, Kansas, Nebraska, Wyoming, and California have collections of varying size and importance. The South Dakota State School of Mines, at Rapid City, has a wonderful collection of White River mammals, probably the finest in existence, in quality, if not in quantity.

It is in the public museums which now dot the country from Boston to Los Angeles that the influence and example of the American Museum have been most obvious. The United States National Museum has built up a magnificent series of vertebrate fossils, as have also the Carnegie Museum in Pittsburgh, the Field Museum in Chicago, and the Colorado Museum in Denver. From the tar-pits of Southern California the Los Angeles Museum has gathered the most remarkable collection of Pleistocene mammals known anywhere in the Northern Hemisphere; La Plata and Buenos Aires are worthy rivals in the Pleistocene of South America. Nor should our Canadian brethren be forgotten, for the Royal Ontario Museum at Toronto and the National Museum at Ottawa contain noble collections of fossil vertebrates, especially of dinosaurs.

Nearly all of this wonderful growth has taken place since 1888, and it is a remarkable achievement in half a century.

A condition precedent to the assembling of these great collections was the immense improvement in the art of collecting introduced by Hatcher, working for Marsh, and Wortman on behalf of Cope. Leidy's material was sent in to him by men who did their collecting incidentally, as other duties permitted, and merely picked up from the surface of the ground fossils that had been weathered out of the soft Badland rocks. Save for a few finely preserved skulls, this material was very fragmentary, and Leidy never figured so much as a foot, to say nothing of a skeleton. Indeed, much of the fossiliferous area was dangerous on account of the Indians, and Dr. Hayden owed to his reputation for madness the immunity which permitted him to explore the White River Badlands of Dakota and Nebraska. Not only was such collecting unsystematic, but a great deal of it was stratigraphically inaccurate and led

to a confusion as to the classification of the continental Tertiary rocks of the Great Plains, which is only now being cleared up.

Hatcher and Wortman, on the other hand, devised and perfected methods of collecting by means of which the fossils were brought back in as perfect condition as when they lay in the strata. Great numbers of entire skeletons were thus secured, from the huge dinosaurs which weighed many tons to tiny lizards and mammals whose weight is computed in ounces. The most ponderous and massive to the most delicate and fragile fossils are thus brought home intact. It cannot be too emphatically stated that the paleontologist is dependent upon the collector for the material of his studies.

Another feature of our half century has been the growth and expansion of collecting expeditions, which had already begun in a small way. Aside from the various geological and geographical surveys, supported by the National and State governments, which made collections of fossils incidentally and as opportunity offered, the earliest of these expeditions would seem to have been the collecting parties of students taken out from Yale by Professor Marsh in 1871-1873. Next in time came the 1877 expedition from Princeton, first of a series still continuing. It is a source of great gratification to me that I was concerned in originating and planning that expedition, in view of its far-reaching consequences, for the work done by the parties of 1877 and 1878 was the means of diverting Osborn's interests into the channel of his great career. As long as he lived, he looked back with keenest pleasure to those early days in primitive, largely unknown Wyoming. In a letter written in 1933 from the field in Montana, when gasoline did all the work from transportation to cookery, he told how he missed and regretted the old equipment—the saddle horses, the "chuck-wagon" with its team of mules, and, above all, the evening campfire and its good comradeship. Those conditions, obsolete and forgotten as they now are, were far more interesting, if less efficient, than the modern motor car and airplane. As Matthew put it, the romance of fossil hunting had fled from the Ford car and the barbed-wire fence.

The work of collecting by student and professional parties, from the colleges and public museums, has continued unabated to this day, but the half century has witnessed the extension of enterprise to distant lands and other continents. The first of these that I recall was the Princeton expeditions to Patagonia in 1896-1899, which were originated, planned, and executed and largely financed by Hatcher, who was fascinated by the discoveries of Ameghino in the Santa Cruz Miocene. The third and last of these expeditions was shared by the American Museum, which thus began the series of foreign exploration and collecting parties, which have

so greatly added to the fame of that institution. In addition to the South American work, which is still going on, were the exploration, geological and paleontological, of the Fayûm in Egypt, of the Siwalik Hills of northern India, and, above all, the Gobi Desert of Mongolia and other regions in China, to which several years were devoted. Yale University has sent expeditions to the Siwaliks, and Harvard to Australia and Brazil. Amherst was successful in Patagonia. Especially brilliant in results were the expeditions of the Field Museum to South America, where parties were kept in the region for several years, collecting not only in the Santa Cruz but in the Pliocene of Catamarca and the Tarija of Bolivia. Extremely valuable and instructive has been the work of the American Museum's Searritt Expeditions in the Oligocene and Eocene of northern Patagonia.

So closely interwoven has been the vertebrate paleontology of North and South America that something must be said of the museums and the workers of Argentina, which has almost a monopoly of vertebrate fossils earlier than the Pleistocene of Bolivia and the Brazilian caverns. The two leading museums are those of Buenos Aires and La Plata, the former chiefly the work of Burmeister, whose career lies outside of our time limits, and the latter founded by Don Francisco Moreno, whose feud with Ameghino was curiously like that between Marsh and Cope. The latter used laughingly to say that the paleontological situation in Argentina was a close parallel of that in the United States, Burmeister representing Leidy, Moreno being Marsh, and Ameghino Cope. It is curious to find so near an analogy in so distant a land, with an alien culture and language.

I trust you will pardon my indulging in some personal reminiscences at this point, for they seem necessary to the explanation of certain obscure and tangled conditions. The work of determining and describing the immense collection of Santa Cruz mammals and birds made by Hatcher and Peterson had been entrusted to my lamented colleague Sinclair and myself, and I soon found it necessary to visit Argentina and study the type specimens, which had been described, but very inadequately illustrated. In those days, one had to travel by way of England, and in London I had long consultations with Lydekker and Smith Woodward, who had visited La Plata and knew the situation. They told me that Moreno was then in London and advised me to call on him and explain my plans. He received me with the utmost courtesy and kindness, gave me letters to his heads of departments and invited me to take up my quarters in his official dwelling in the museum building. Moreno's absence from La Plata was a very fortunate circumstance for me, for it enabled me to make use of Ameghino's collection and that of the museum; had he been at home, I should have had to choose between them, so bitter was Moreno's feeling toward his rival.

I do not know the merits of the quarrel and should not like to take sides as between two men, each of whom had been exceedingly kind to me, and, happily, it is my good fortune that I was in a position to ignore the matter, save on one memorable occasion.

I had gone to La Plata strongly prejudiced against Ameghino by what I had heard from Hatcher and Lydekker, who had engaged in rather acrimonious controversy with him and had not emerged victorious. To my surprise, the staff of the La Plata Museum had no such feeling, and it was one of them who took me to Ameghino's house and presented me; my welcome was most cordial, and in that house I spent nearly every afternoon for the next four months, Sundays excepted. Ameghino's treasures were most freely placed at my disposal, even undescribed material, of which, ordinarily, he was extremely jealous. My opinion of him underwent a rapid and complete metamorphosis; not only did I come to have a warm personal liking for him, but also, the farther I went in my own problems, the more did I admire his achievements, which, both stratigraphically and paleontologically, were pioneer work of a high order. Unfortunately, he was subject to certain obsessions, which marred the value of his later papers, but it was easy enough to make due allowance and discount the results. He was, in the first place, in the habit of making the formations in Patagonia far too old, geologically speaking, and correlated Tertiary beds with the Cretaceous of North America and Europe and, secondly, he was persuaded that all types of mammals, even including Man, originated in Argentina, a belief which produced some fantastic genealogies. However, these excrescences were superficial and did not affect the real value of his work, which has grown all the greater with the progress of time.

Ameghino's career is one of the most noteworthy in the history of Science for heroic devotion to his chosen work in the face of disheartening difficulties. A professor in the University of Córdoba, Moreno brought him to La Plata as assistant director of the newly established museum. Almost immediately, the great quarrel broke out, and Ameghino was ejected without a position, literally turned into the street. Undiscouraged, he set up a stationery shop and supported his wife and himself, living like hermits in one corner of the patio, while the remainder of the house was devoted to his collection. Every penny that he could scrape up went to keep his brother Carlos collecting in Patagonia, and a windfall of $25,000 kept his *Revista Argentina de Historia Natural* going for a year. It is a fortunate circumstance that Carlos Ameghino was a stratigrapher of no mean ability and his work has stood the test of time and re-examination in the field.

To anticipate, it is gratifying to know that the closing years of Florentino Ameghino's life were made easy by his appointment to the directorship

of the National Museum in Buenos Aires, the highest scientific post in the Republic, and made happy by the admiring recognition of his fellow countrymen, who were inordinately proud of his distinguished position in the world of Science.

A large part of my work in La Plata was the photographing of Ameghino's types. Some 600 pictures were made, which I had to develop and print myself, owing to the press of emergency work at the Museum, where likewise an important series of photographs was made. I brought home with me the glass negatives, and these have proved very useful to several workers in South American paleontology.

As I have said, Lydekker was very critical of Ameghino, who felt keenly the famous Englishman's failure to call on him during the two visits which he made to the La Plata Museum. There he made studies, chiefly of Santa Cruz fossils, and these studies were published in the folio reports of the Museum. Before leaving London, I asked Lydekker why he had not visited Ameghino, in whose collection he would have found much better material than that at the Museum. He replied: "I couldn't; I was in La Plata as Moreno's guest and he was paying my expenses; he would have sent me home on the next ship, if I had called on Ameghino." One day, when the latter was speaking sorrowfully of Lydekker's treatment of him, I thought it would be no violation of confidence to repeat that conversation; Ameghino's relief was pathetic: "Oh!" he said, "I am *so* glad to hear that; I thought he meant to put a deliberate slight on me." He added: "I am extremely glad that you have come to work in my collection, for you can give impartial evidence that I have worked honorably."

At the time of my visit, the curator of vertebrate paleontology in the La Plata Museum was Santiago Roth, an immigrant from Switzerland and a self-taught paleontologist. Like all the other officials he was most kind and helpful, and I acquired great admiration for his work. The paper in which he proposed the superorder Notoungulata is a classic and found immediate acceptance the world over.

Altogether, my experiences in Argentina were indescribably valuable and of fascinating interest.

What has been the outcome of all this enterprise and zeal in the gathering of fossils from the ends of the earth and building museums in which to store and display them? Manifestly, it would be impossible to present here catalogues of the great collections, and, even were it feasible, it would be unpardonably dull. Results must be expressed in comprehensive terms. First of all should be mentioned the Dinosauria, which throughout the Mesozoic Era had dominated all the continents, for none of these monsters were marine, though some may have frequented fresh water. The dinosaur hall in the American Museum is the most astonishing assemblage of these

frightful creatures—gigantic or tiny—that has ever been brought together, though the collection at Yale, in Washington, Pittsburgh, Toronto, and Ottawa are only a little less impressive. The Dinosaur National Monument in Utah is a display that is unique in the history of our science.

Many other types of reptiles have been brought to light; the sea-dragons or mosasaurs, ichthyosaurs, and plesiosaurs are unending sources of wonder, especially the ancestral types found in the Trias of Nevada. Quite recently, the capital discovery of the ancestral Triassic crocodile was announced by Mr. Barnum Brown of the American Museum, thus clearing up a mystery of long standing, which Huxley had in vain endeavored to pierce.

Of the gigantic flying dragons, or Pterosauria, of the Kauras Chalk, many fine specimens have been unearthed.

Among birds Marsh's astonishing discovery of the tooth-bearing kinds was made before the beginning of our period, and nothing so startling has been brought to light since, but some extraordinary finds have been made within that time. The gigantic Eocene bird, which Cope named *Diatryma* from fragments, is now known from skeletons, and the great flightless birds of the Patagonian Miocene are, structurally at least, well known. Ameghino's *Phororhacos* had a skull larger than that of a horse.

Among mammals, the list of new and significant discoveries is unending; the Paleocene faunas, especially of Montana, Wyoming, and Colorado, have been multiplied many times, and the extremely surprising discovery of North American edentates in the Eocene was announced by Osborn and Matthew. Other discoveries in the Bridger have been chiefly the finding of skeletons of genera which had been known only from a few teeth or a jaw fragment. The American and National Museums have been particularly successful in obtaining this beautiful material. In 1888 very little was known of the upper Eocene, or Uinta stage, which had been discovered and named by Marsh a dozen years before. The work of the Carnegie Museum, in the first place, and the American and Field Museums, in the second, has unearthed a host of new genera and perfect specimens of many more that had been previously known, though very inadequately.

The new fauna of the Duchesne River was discovered and exploited almost exclusively by the Carnegie Museum parties under the leadership of the late O. A. Peterson. The Sespé of California, the lower part of which contains a fauna like that of the Duchesne River, is the discovery of the California Institute of Technology at Pasadena.

The Eocene and Oligocene faunas and their order of succession, that of the Duchesne River and the Sespé excepted, were already known and unanimously accepted in 1888. Since then the work in them, magnificently successful as much of it has been, is the task of the collectors. The splen-

did achievements of the American Museum in Mongolia are hardly relevant and yet they cannot be passed over without mention, for, in the Eocene and Oligocene, eastern Asia and western North America were parts of the same zoölogical province, and the Gobi faunas are more akin to America than to Europe. The Miocene and Pliocene faunas of the West were an altogether different matter, and the utmost confusion prevailed in the Tertiary geology of the Great Plains from the Dakotas to Texas and Mexico. Little by little this veil has been lifted, and now the Tertiary succession is well established, the only differences of opinion being as to the exact position of the boundaries Eocene-Oligocene, Oligocene-Miocene, and Miocene-Pliocene, so gradual is the passage from one to the other.

Out of this welter of confusion have come an immense number of new discoveries, of which only the most striking and conspicuous can find room here. Of these, undoubtedly the most spectacular are the Proboscidea, which increased and multiplied and diversified through the upper Miocene, Pliocene, and Pleistocene to an incredible degree. The array of mastodont and elephant skeletons in the American Museum and the Morrill Museum of the University of Nebraska is simply astonishing, not only for their perfection of preservation, but for their extraordinary variety and, near the end, for their colossal size. *Archidiskodon imperator* and *A. maibeni* are among the largest of known elephants and the largest land mammals that ever lived in the Western Hemisphere. Among this great assemblage there may have been some indigenous genera, but most of them arose in the Old World and reached this continent in wave after wave of immigration, gradually spreading through Mexico and Central America into South America and ranging over that continent down to Argentina and Bolivia.

Another remarkable group—the clawed perissodactyls which Cope named the Ancylopoda—falls strictly within our period, so far as comprehension of them is concerned, for the first discovery and naming of their fragmentary remains dates back to Cuvier and Lartet. It was in the summer of 1888 that Forsyth Major first suggested that the typically perissodactyl *Chalicotherium*, known only from the skull and teeth, had the feet of *Ancylotherium*, which was believed to be an edentate, the "pangolin gigantesque." This conjecture was speedily proved to be true by Filhol's discovery of a connected skeleton at Sansan in the south of France.

In this country the course of discovery was exactly parallel; Marsh had described a foot, which he called *Moropus* and classed with the gigantic ground sloths. Early in the present century, Hatcher and Peterson, working in the wonderful "bone-bed" of the lower Miocene at Agate, Nebraska, discovered whole skeletons of *Moropus*, which proved to be one of the

clawed perissodactyls—or Ancylopoda—and nearly allied to the European *Chalicotherium*. Indeed, the teeth of *Moropus* found in Nebraska were in 1886 assigned to *Chalicotherium*, and any suggestion that they might be associated with feet like those of *Moropus* would have been laughed to scorn.

In 1890 Hatcher secured for Marsh from a collector, Welles, a skull found in the White River Badlands, which he called a "horned *Oreodon*." This skull was subsequently described by Marsh under the name of *Protoceras* and proved to be a fantastic creature with two pairs of horn-like protuberances, the early number of a line which ran through the Miocene (*Syndyoceras*) growing ever more bizarre and culminating in the ridiculous *Synthetoceras* of the lower Pliocene.

Finally may be mentioned two families of ruminants, the overwhelming development of which in the Miocene, Pliocene, and Pleistocene has only lately been brought to light by Mr. Childs Frick, through intensive collecting in relatively barren areas. It has long been recognized that the prong-horn (*Antilocapra*), which is so exceptional that it has been put in a family by itself, must be the last surviving remnant of a once extensive group. This group has now been revealed in a surprising wealth of forms, some of them with twisted horns, like the strepsicerine antelopes of Africa, and many four-horned and more or less grotesque in appearance.

The other ruminant series belongs to the deer family but is a cycle of peculiar types unlike anything now living and characterized by the great length of the pedicles and reduction of the antlers proper, somewhat as in the modern Muntjak, but in a very exaggerated way. Like the prong-buck family, these aberrant deer included three- and four-horned kinds and represent, so far as is now known, an exclusively North American development.

The vertebrate groups which I have mentioned are not to be taken as a catalogue, but as a few examples of the more striking, not to say sensational, discoveries of the last half century.

All this enormous and immensely costly apparatus of collections and museums would be wasted and meaningless without investigators to make use of them. The little band of half a dozen workers, like Williston, Baur, and Osborn, who were to replace the great pioneers of the preceding generation, has grown into a large company. The Society of Vertebrate Paleontologists now counts some 70 members and is made up of active workers busily engaged in describing and evaluating the treasures which the collectors have gathered. Mere description, however indispensable as a preliminary, is insufficient; the biological, geographical, and stratigraphical significance of the fossils is what gives value to the paleontologist's work. It is impossible to give a catalogue of the Society's membership, with an

account of the researches on which each one is engaged, and selection of a few names would be invidious and unjust to the others, save on the ground of seniority. So far as the living are concerned, the preceding rapid outline sketch of discovery and exploration has been almost anonymous, but in what follows it will be necessary to name some of the veterans of our science.

Activity in research is measured by publication, though of course that is not a very accurate standard, for quality is so much more important than quantity. The American literature of vertebrate paleontology published since 1888 is an immense body, as may be estimated by a glance through Hay's two bibliographies, and, happily, the volume is ever expanding. The paleontological series of the *Bulletin* and *Novitates* of the American Museum make a stately row of volumes, and even more impressive are the Reports of the Asiatic Expeditions. Dr. Granger's discovery of the dinosaur nests and eggs in the Gobi resounded throughout the world, even in the conservative pages of *Punch*. The 15 quarto volumes of Reports of the Princeton University Expedition to Patagonia, seven of them devoted to paleontology, the publication of which was made possible by the generosity of the late Mr. Pierpont Morgan, perpetuate the brilliant success of Hatcher and Peterson. These volumes are, of course, the work of many hands. The quarto Memoirs of the Peabody Museum, Yale University, began in 1880 with Marsh's famous monograph on the Toothed Birds and has continued to the present in works of high distinction. One of these was Dr. G. G. Simpson's remarkable monograph on the American Mesozoic Mammalia which, together with a similar volume on the Mesozoic mammals of the British Museum, has thrown a flood of light on places that were very dark. Professor E. H. Barbour's papers from the University of Nebraska have recorded his epoch-making discoveries of elephants and mastodons, to say nothing of smaller but no less curious creatures, such as the incredible *Syndyoceras*. Dr. J. C. Merriam made famous the museum of the University of California with his researches on the Mesozoic reptiles and Tertiary mammals of the Pacific Coast region. As President of the Carnegie Institution of Washington, he has found time for the series of the Carnegie *Contributions to Paleontology*, which are so valuable to the worker.

Osborn's published works fill a bookcase and number over 400 titles, less only than those of Cope, and publication did not end with his life. The first volume of his magnificent monograph on the Proboscidea was published nearly a year after his death, and the second, still in course of revision, is yet to appear. His other monumental work, the two quarto volumes on the Titanotheres, was brought out by the United States Geological Survey.

The late Dr. W. D. Matthew, also a very prolific writer, left behind him a monograph on the Paleocene mammals of New Mexico; this was brought up to date by Dr. Walter Granger and published as an imposing quarto in the Transactions of the American Philosophical Society.

Again, it must be emphasized that this is very far from being a catalogue of paleontological publications but includes merely a few specimens, taken, almost at random, from a great body of literature. Anyone who wishes to know how great that literature is need only look over the three thick volumes of Hay's bibliographies.

The primary work of the vertebrate paleontologist is zoölogical; he strives to learn the structure, classification, and evolutionary history of the extinct creatures with which he deals, as well as their migrations and geographical relations. He has, further, a profound interest in stratigraphy, for that branch of geology gives him the chronological succession, without which his fossils are meaningless. The continental Tertiary deposits of the West and the Pacific Coast region are classified according to the contained fossil vertebrates, which must, however, be checked and controlled by the observed order of superposition. Faunal horizons are not always the same things as lithological units and must be carefully discriminated. The Miocene and Pliocene of the Great Plains have gradually been classified by the combined use of both methods, and, at last, a definitive result seems to have been attained. Fifty years ago hardly anything was known of American continental Pliocene; now it is one of the most completely documented parts of the Tertiary column. The vertebrate paleontologists have named and defined many new subdivisions of the continental Tertiary rocks: Cope's Puerco and Blanco, Marsh's John Day and Uinta, Peterson's Duchesne River, and Hatcher's Harrison and Monroe Creek are some of the many instances of this. The discovery by Douglass of mammals in the Fort Union settled the controversy over the geological correlation of that great unit, and the gradual working out of the Paleocene problem and its units has been accomplished by the students of vertebrates. The Morrison and the Lance are Mesozoic analogues.

In conclusion, a word must be said concerning the work which the paleontologists have accomplished through the museums in educating public opinion. It would be absurd to claim that the radically changed attitude toward Science on the part of the newspapers and the public has been mainly due to paleontologists. Chemists and physicists have achieved most in this respect, astronomy has always been popular, and its professors have been treated with respect. Nevertheless, the public museum and its vertebrate fossils have been large factors in this liberalizing work. In 1879 Osborn published in the *American Journal of Science* a small paper on the jaw and teeth of an Eocene mammal; one of his separates found its

way into the office of a New York daily newspaper, which proceeded to make unlimited fun of such a ridiculous performance. Even so late as the 1890's when the first dinosaurs were exhibited in the American Museum, the newspapers thought that they were topics for the comic strip rather than the news columns. Now, every school child in New York is familiar with dinosaurs. The people in Rapid City (South Dakota) are so proud of the remarkable collection of fossils in the State School of Mines that the taxi-cab drivers recommend it to arriving strangers as something they ought to see. Every year tens of thousands of school children visit the American Museum, which is formally a part of the public school system of New York. The children do not ramble aimlessly about but are marshalled in orderly bands by teachers who are competent to explain to their pupils the strange things that they see. In Philadelphia the Academy of Natural Sciences is doing a very similar work.

"The conclusion of the whole matter" is summed up in a phrase of a letter written me by Sir D'Arcy Thompson, of St. Andrews, after his return from a visit to this country last year. "You have to look back, for one thing, on the growth from small beginnings of a great science." That is the achievement of the half century.

It is for lack of time that I have omitted mention of the great paleontologists of Europe, many of whom it was my privilege to know personally. Traquair, Huxley, Sollas, Smith, Woodward, Lydekker, Andrews, Broome, Stirling, Dollo, Abel, Zittel, Schlosser, Stromer, V. Huene, Nopcza, Rütimeyer, Stehlin, Fraas, Kiaer, Stensiö, are an illustrious company beyond all praise.

PRINCETON, N. J.
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REGIONAL DEPARTURES FROM IDEAL ISOSTASY

BY REGINALD A. DALY

(Anniversary Day Address)

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INTRODUCTION

The principle of isostasy means two things. First, it implies that an earth-shell beginning at a moderate depth from the surface of our planet is so weak that the material of the shell is nearly or quite in the hydro-

static state. For this weak shell Barrell coined the name "asthenosphere". The word says literally that the shell has no strength whatever, but Barrell did not believe that it has no strength and invented the name merely to indicate relative weakness. Secondly, the principle declares that high segments of the overlying shell—the lithosphere—are in large part hydrostatically balanced by the low segments, the rugged relief of the globe being in other parts supported by the strength of the lithosphere.

It is also important to observe that isostasy does not necessarily imply a hydrostatic condition for any shell below the asthenosphere. The truth of the principle would not be affected if it were proved that notable strength and permanent shearing stress exist in one or more of the deeper layers—those of the centrosphere.

Any balancing of high and low segments of the lithosphere must be due to an appropriate distribution of densities for the lithospheric material. The worth of the isostatic theory has been tested by the use of three different assumptions regarding this distribution. According to the Pratt-Hayford hypothesis the density changes uniformly with increase of depth, in such a way as to ensure balancing of all vertical columns within the lithosphere. The Airy hypothesis assumes: (1) Continental and mountain roots of Sial; (2) hydrostatic support for the relief by the pressure of the Sima, into which the roots are plunged; (3) uniform density for the Sial; and (4) uniform and higher density for the Sima. The third hypothesis, developed by Heiskanen, postulates Sialic roots and supporting medium of Sima, with, however, vertical increase of density in Sial and also in Sima. As now generally phrased, these root hypotheses—the "Airy" and "Heiskanen-Airy"—recognize that the upper part of the Sima, like the Sial, has considerable strength.

The mathematical testing has been simplified by making an additional assumption—that each feature of the earth's relief, no matter how minute, is compensated by an appropriate density for the rock immediately underlying that feature and extending down to the top of the asthenosphere. All investigators, however, have fully appreciated that the isostatic compensation cannot be local in this sense. They have known from the start that the compensation must be regional. Yet the most convenient way to test isostasy with regional compensation has been to base computations on the data derived from the clearly erroneous premise of local compensation. Furthermore application of both the Airy and Heiskanen hypotheses of local compensation has been made relatively easy because Hayford and Bowie, with the help of many colleagues in the United States Coast and Geodetic Survey, had already prepared the labor-saving tables used for testing the hypothesis of

local compensation (W. Heiskanen, 1924; 1926; 1932). Hayford, that organizing genius, devised a practical mode of procedure in the case of the United States, where observations on deflections of the plumb-line and on the anomalies of gravity were to be used. On the present occasion we are to be concerned chiefly with the so-called Hayford anomalies of gravity—namely, those computed on the assumption that the lithospheric densities are distributed according to the Pratt-Hayford idea, and on the second assumption that the isostatic compensation is purely local.

Bowie in the United States, Heiskanen in Europe and Japan, Glennie and others in India, Cassinis in the Mediterranean Sea, and Vening Meinesz over the oceans have shown that there are wide areas of positive Hayford anomaly and wide areas of negative Hayford anomaly. This is true when the observed values of gravity are systematically compared with the theoretical values calculated for the same stations when the 1930 International formula for the figure of the earth is used. In general the broad areas of one-sign anomaly keep their signs though not quite the same values for the mean anomalies, even when the anomalies deduced on the Airy, Heiskanen, or corresponding regional hypotheses of isostatic compensation are mapped. The geological significance of the broad fields may therefore be approached through a study of the Hayford anomalies, notwithstanding the fallacy of assuming a local character for the isostatic compensation.

THE QUESTIONS TO BE REVIEWED

In some instances the Hayford anomalies in the fields described can be explained as due merely to local abnormalities of rock density near the surface, and to that extent such fields need not mean departures from isostasy. But in general the broad fields suggest actual excesses or defects of mass in the corresponding sectors of the earth. The specific problems to be here discussed are: first, how great are the loads so represented? and second, what is the distribution of the stresses caused by those loads?

From his pioneer study Barrell (1915, p. 33) concluded that the stresses are borne by the lithosphere, the asthenosphere, and the underlying centrosphere. Jeffreys (1929, p. 230) thinks the stresses are borne by the whole of the silicate "mantle" overlying the "iron core" of the earth. Gilbert (1914, p. 35) suggested that they may be borne essentially by the lithosphere and centrosphere, the asthenosphere being, what its name literally implies, a layer incapable of withstanding prolonged shearing stress.

Recently published gravity anomalies in the glaciated tracts seem to indicate extreme, if not perfect, weakness of the asthenosphere. On the other hand, the growing tables of anomalies in the United States, over

the oceans, and in India appear at first sight to negative that conclusion. Is it possible to bring the two sets of facts into agreement? This question is manifestly of deep meaning for a proper understanding of orogeny, epeirogeny, igneous action, and isostatic adjustment itself. First will be noted the testimony of the glaciated tracts and then of India, where the distribution of strength along the vertical has seemed to be in special contrast with the distribution along the vertical in the glaciated regions. A possible way of reconciling the conclusions from the two sets of facts will be described.

Four other types of reported regional departures from isostatic balance will be briefly discussed: (1) The fields of positive isostatic anomalies over the deep water of the Pacific and Atlantic oceans; (2) the offshore, coastal belts showing algebraic addition to the isostatic anomalies as the depth of the ocean there increases; (3) the ocean Deeps; and (4) the basins occupied by the mediterranean seas, which give large and systematically positive anomalies.

The writer has pleasure in acknowledging data supplied by Professors Francis Birch and H. M. Westergaard of Harvard University, by Colonel E. A. Glennie, Dr. J. de G. Hunter, and other officers of the Survey of India, and by Dr. F. A. Vening Meinesz of the Netherlands Geodetic Commission.

ISOSTATIC RECOIL IN DEGLACIATED REGIONS

Raised beaches and warped beach-lines in ten different sites of vanished or greatly diminished ice-caps prove post-Glacial upwarps of the lithosphere. Some of these upwarps are generally thought to be due to isostatic recoil from the basining which had been caused by the respective loads of ice, and it is probable that all of the ten cases are to be explained in the same way. Smaller ice-caps, like those that overwhelmed Kerguelen Island and the Faroe Islands during the Pleistocene, seem not to have basined the lithosphere non-elastically. If this is true, we have an idea of a minimum value for the bending strength of the lithosphere. The weights of the larger ice-caps did bend down the lithosphere plastically, and the recoil after deglaciation still continues both in northwestern Europe and northeastern North America. Recently published measurements of gravity within these two regions of recoil appear to permit estimates of the maximum possible strength of the asthenosphere. The Fennoscandian case will be considered first.

Combining Witting's map, which gives the present rates of upwarping in the Baltic area, with the map of isobases for the Littorina stage of post-Glacial time, Penck (1922, p. 305) has drawn the map of Figure 1. It shows, probably with fair approximation, the rate of upwarping for the

glaciated tract as a whole. Near the center of the elliptical area and also near the point where the ice was thickest, the uplift is most rapid. If the recoil is due to isostatic adjustment, there must be deficiency of mass under the glaciated tract and at maximum near the center of the

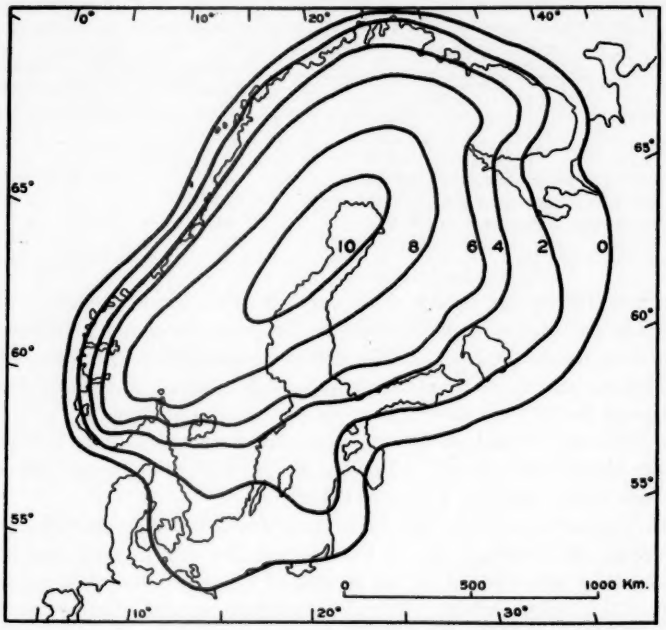

FIGURE 1.—*Map showing present rate of uplift of Fennoscandia*
Figures on contour lines give rates of uplift in millimeters per annum.

tract. The obvious means of estimating the deficiency is by the use of gravimeters. It is to be hoped that the rapidly worked and apparently accurate static gravimeters of the Lejay, Haalck, or Thyssen types will soon be applied to all Fennoscandia; in the meantime it is worth while to see what light the measurements of gravity with the pendulum can throw on the problem, even though the network of stations occupied covers no more than one third of the Fennoscandian tract.

Hirvonen (1937) published the Free-air anomalies at 202 Finnish stations and 56 Russian stations, all well inside the terminal moraine of the last glaciation. The innermost stations are located not far from the center of the tract. From station to station the anomalies change sign irregularly, without manifest cause, but, when simple averages for belts

concentric with the locus of maximum upwarping are calculated, these averages are found to vary systematically with distance from that central locus. The results of the computation are given in Table 1.

TABLE 1.—*Gravity anomalies in glaciated tract of northwestern Europe*

Belt	Number of stations in belt	Free-air anomalies (milligals)	Hayford anomalies (milligals)
From center to radial distance of 300 km.....	40	−14.5	−17
Between radial distances of 300 and 600 km.....	129	− 5.5	− 8
At radial distances greater than 600 km.....	89	+ 1.4	− 1

As expected on the theory of isostasy, the Free-air anomalies in this relatively flat region increase algebraically from the center of the tract toward the terminal moraine. The Hayford anomalies are obtained from the Free-air anomalies by adding about −2.5 milligals to each of these; the results are shown, in average, in the last column of Table 1.

At the actual center of the tract the Hayford anomaly is doubtless smaller algebraically than −17 milligals. A value of −20 milligals will be taken as the basis for the following discussion.

Let it be assumed: (1) That the isostatic recoil is accompanied by pure upbending (no faulting) of the lithosphere; and (2) that the uplift at any point is proportional to the weight of ice which had lain on that point of the rocky surface. When of maximum volume, the ice-cap had profiles closely matching those of a semi-ellipsoid with horizontal diameter about 1500 kilometers in length. Now the maximum shearing stress due to such a load, positive or negative, on a perfectly homogeneous earth-shell of great thickness can be estimated from a diagram in a treatise by Timoshenko (1934, p. 350), shown in Figure 2. Accordingly the maximum stress under the center of the semi-ellipsoid would be about 30 per cent of the pressure (P) exerted at this center and would be situated at a depth equal to about 78 per cent of the long radius of the ellipsoid. A central anomaly of −20 milligals means a negative load, numerically equal to the weight of 180 meters of granite and equal to an upward pressure of about 48 kilograms per square centimeter or 48 metric atmospheres. Taking the effective radius of the glaciated tract at 750 kilometers, and for the moment making the inadmissible assumption that all material below the ice-cap is homogeneous with respect to elasticity and strength, the maximum shearing stress under the center of the cap

is seen to be approximately 14 atmospheres and is located at the depth of about 600 kilometers.

From Timoshenko's curve the maximum stress-difference (shearing stress) at other depths can be read off, with the results shown in Table 2.

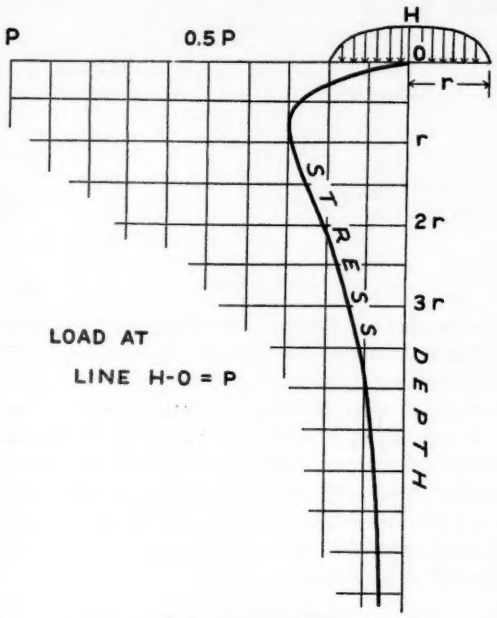

FIGURE 2.—*Distribution of maximum stress-difference under the center (O) of a semi-ellipsoidal load with radius r.*

Thus there is reason to think that the maximum shearing stress at the depth of 100 kilometers cannot now be more than 3 atmospheres. When the deep, isostatic flow comes to an end, perhaps 10,000 or 20,000 years hence, the residual stress-difference at this depth will be smaller still. Because the lithosphere is strong, it should not yield perfectly, so as to annul all of the deficiency of mass in the glaciated sector of the globe, but it seems quite possible that after the lapse of a few tens of thousands of years the asthenosphere shall have become freed of all shearing stress of the kind here considered.

In any case this glance at the situation in northwestern Europe suggests a strength for the asthenosphere which is much smaller than that deduced

by Barrell or by Jeffreys. Barrell (1915, p. 33) estimated the elastic limit of the material at the depth of 100 kilometers to be between 200 and 300 atmospheres; the estimate of Jeffreys (1929, p. 230) is of the order of 100 atmospheres.

TABLE 2.—*Approximate shearing stress under the center of the glaciated tract of Fennoscandia*

Depth (kilometers)	Stress-difference (atmospheres)
0	0
100	3
200	8
400	10
600	14
1200	ca. 10
1800	ca. 5

It is significant that the Hayford anomalies in the glaciated tract of northeastern North America also decrease algebraically as the center of the tract is approached. For example, the mean of 115 of the anomalies (again with regard to sign) between the terminal moraine and the Canadian border is —4 milligals, while the mean for 25 Canadian stations (all hundreds of kilometers from the center of this glaciated region) is —20 milligals. Evidently new lake-gage, tide-gage, and gravity investigations are here needed, but, so far as the facts in hand go, they too seem to spell much greater weakness for the asthenosphere than that calculated by Barrell or by Jeffreys.

ISOSTASY IN INDIA

Although the principle of isostasy was first well formulated by Airy and Pratt when they studied the deflections of the plumb-line in India, the geodesists of this very country are now among the most convinced doubters that the principle is valid, except in the sense that continents and regions of subcontinental extent are isostatically balanced by the oceanic segments of the lithosphere. Those authorities are also ready to accept the view that the unbalanced loads with spans greater than a few hundreds of kilometers represent only moderate pressures exerted on the lithosphere. The grounds of their scepticism regarding local compensation are firm grounds. Admirably selected and comprehensive networks of plumb-line and gravity stations have proved beyond cavil that peninsular India embodies two broad areas of negative anomaly with an intervening, broad belt of more positive anomaly—the belt bearing the classic names "Hidden Cause" and "Hidden Range."

The northern area of negative anomaly is largely underlain by the relatively light sediments of the Indo-Gangetic plain (Fig. 3), and in part the prevailing negative anomaly there can be explained by the low density

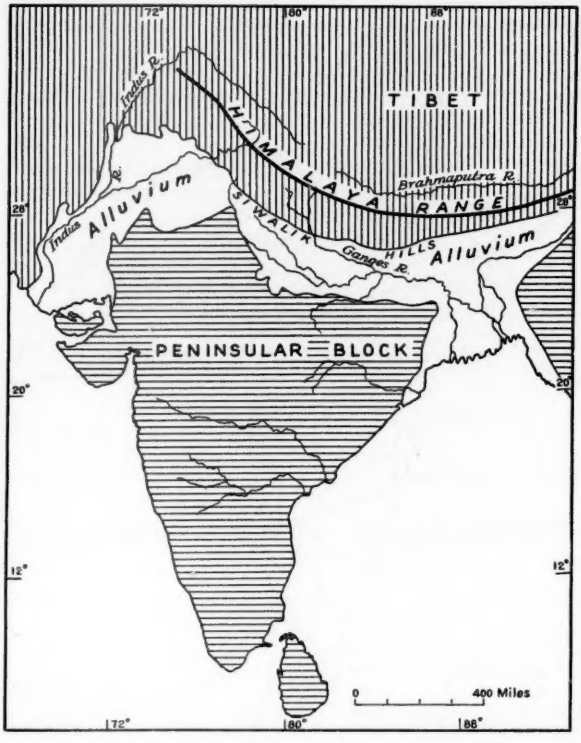

FIGURE 3.—*Principal tectonic provinces of south-central Asia*

of this famous trough of geologically young sediments. But another large part of the collective anomaly seems best referable to an actual deficiency of mass in the earth-sector involved. Still more clearly the negative area on the south, where thick, light sediments are in almost negligible quantity, appears to mean a sector with deficiency of mass. On the other hand, the Hidden Range is a locus of excess of mass.

The necessity of recognizing this three-fold division of the gravity field is evidenced by the Indian Survey's contour maps of the "natural" and nearly identical "compensated" geoids. Figure 4 is a copy of the

first-named map, the contour interval being 5 feet. The stippling indicates the area where the geoid stands above the surface of the 1930 International spheroid of reference. The blank areas bearing contour lines

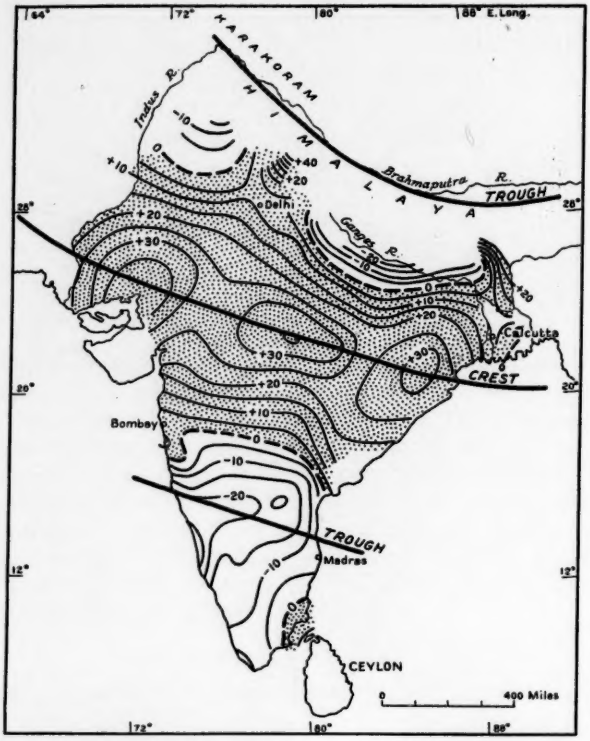

FIGURE 4.—*Map of the "natural" geoid in India*

Positive (Hidden Range) belt stippled; negative belts to north and south. Axes of belts shown with thick lines.

are those where the geoidal surface lies below the spheroidal surface. Glennie drew the three thick lines to represent the axes of the belts of ruling deficiency and excess of mass, the "crest" being the axis of the "Hidden Range."[1] The warped form of the geoid is further illustrated by the upper diagram of Figure 5, copied from Glennie's Professional

[1] *See* Professional Paper No. 27, Survey of India, by E. A. Glennie; and Figure 1 and Chart A in a paper by J. de G. Hunter (1932, p. 42). The axis of the northern trough was placed by Glennie in the approximate position it should theoretically occupy if the whole belt of deficient mass on the north coincides more or less perfectly with the Tethyan geosyncline (crust-warp hypothesis). A part of this

Paper (No. 27).[2] This is a section at right angles to the axes of troughs and crest. The continuous curve gives the deviation of the geoid from the surface of the International Spheroid; the broken curve gives the

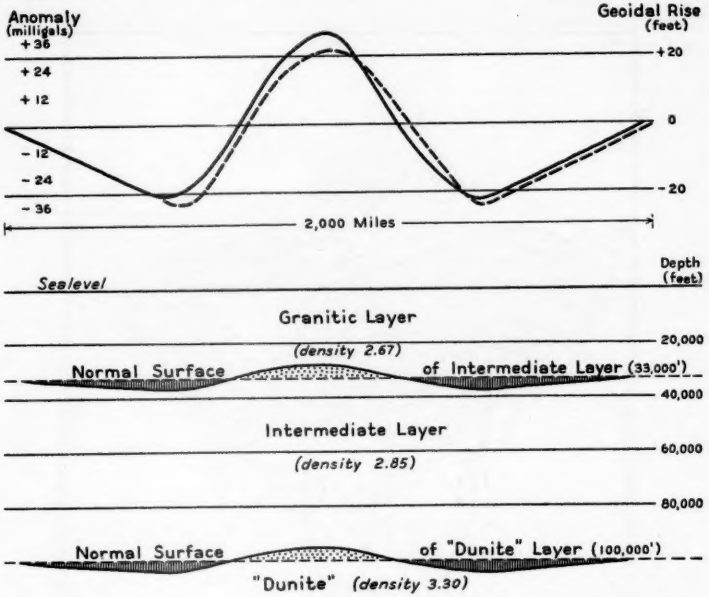

FIGURE 5.—*Section showing warping of the geoid in India; section illustrating Glennie's hypothesis of crust-warping*

Above: Glennie's section across the axes of Figure 4, showing (1) in feet the departure of the geoid from the surface of the International spheroid (1930 formula)—*see* text, with footnote; (2) the corresponding Hayford anomalies in milligals.

Below: A section in the same direction, illustrating Glennie's hypothesis of crust-warping as the cause of the belts of one-sign anomaly in India. Vertical lining indicates loci of deficient mass; stippled areas indicate loci of excess of mass. Thicknesses in feet.

deviation from the surface of the spheroid which is preferred (for India) by the Survey of India and bears the name "Spheroid of India II."

[2] New field observations, reported by Glennie (Survey of India Report, 1934, p. 50), have shown that the geoid is deeply depressed in the extreme south of the peninsula and does not rise sharply as shown in Figure 5.

wider region is included in the negative belt underlain by the Indo-Gangetic alluvium. Only that part is the "northern belt" or "northern area" considered on the present occasion.

Since Figure 4, based on data available in 1923, was drafted and sent to press it was found that newer field observations have led to a re-drawing of the geoidal contours (*see* G. Bomford, Geodetic Report for 1936, Survey of India, chart 7). However, the latest map differs from Figure 4 only in details, the division of the peninsula into the three major belts of one-sign anomaly being in principle unaffected.

Glennie uses the same curve to show the run of the Hayford anomalies in the cross section. Since the peninsula is comparatively flat, essentially a peneplain with monadnocks, these anomalies are not of the kind

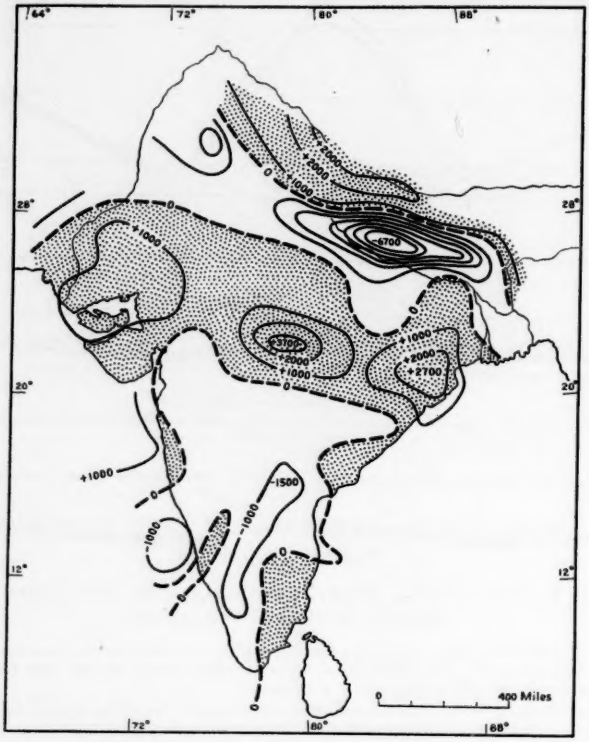

FIGURE 6.—*Hunter's map of mass anomalies which would produce observed warping of geoid in India*

Stippling indicates loci of excess of mass; blank areas with contours are loci of deficient mass.

expected on the hypothesis that the relief is locally compensated. On the other hand, the distribution of the anomalies seems, like the warped geoid, to prove real deficiencies of mass in the two troughs and excess of mass under the surface of the Hidden Range belt.

Hunter (1932, p. 42) has published a map of the mass anomalies which would produce the geoid (Fig. 6). Here the contour lines, with the interval of 1000 feet, give the distribution of the anomalous masses, each

of which is equivalent to so many feet of surface rock with density of 2.67 but uniformly attenuated to Hayford's standard depth of 113.7 kilometers. Hunter also prepared a map of the Hayford anomalies

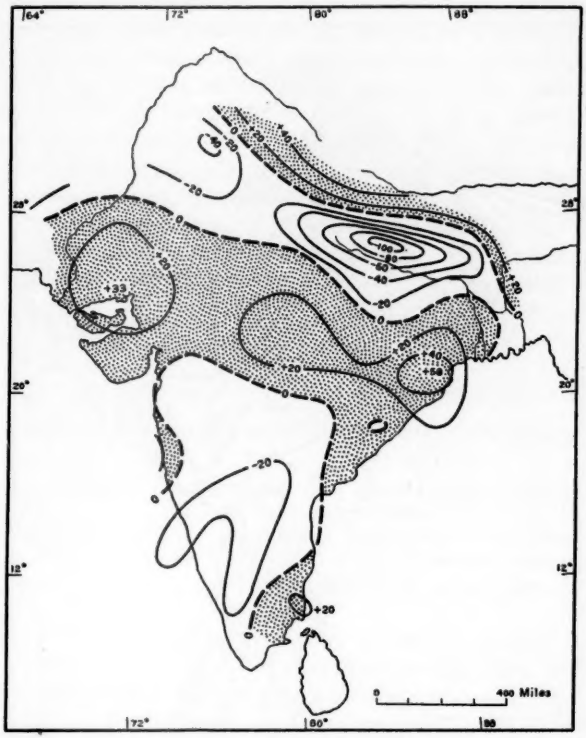

FIGURE 7.—*Hunter's map of Hayford anomalies in India, deduced from geoid*

Areas of positive anomalies stippled.

which were deduced from the geoid (Fig. 7). Once again we see evidence of broad sectors with abnormal masses.

It seems clear, therefore, that if peninsular India is in ideal isostasy, meaning thereby that the asthenosphere closely approaches the hydrostatic condition, the isostatic compensation for relief must be broadly regional; almost all of the stress accompanying the anomalous distribution of mass must then be borne by the lithosphere alone or by the lithosphere and centrosphere together.

That the relatively thin lithosphere alone can bear the stresses implied by the Hayford anomalies based on the International formula may well be doubted. The latest map of these anomalies is given in Figure 8,

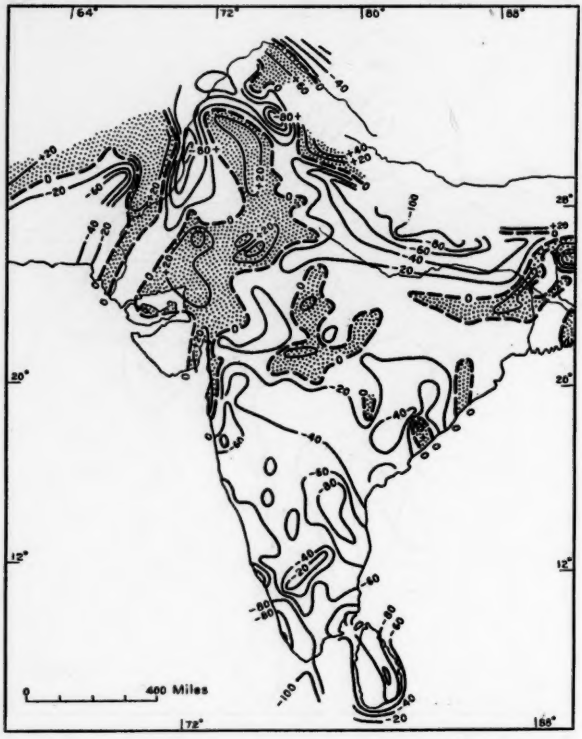

FIGURE 8.—*Map of Hayford anomalies in India, based on the International formula (1930)*

Positive areas indicated by stippling.

where the stippled areas are those of plus anomaly and the blank areas bearing contour lines are those of negative anomaly.[3]

It leaps to the eye that the greater part of peninsular India is of negative anomaly. Moreover, the Hayford anomalies found by Vening Meinesz all across the Arabian Sea, 2500 kilometers in width, are strongly negative. Could this vast, unitary segment of the lithosphere by its own

[3] *See* Chart 11 in Survey of India Report for 1937.

strength resist permanently the upward pressure that matches so wide a field as assumed negative anomaly? There is no evidence that the peninsula is rising because of the pressure. How can such stability be reconciled with the mobility of that other shield area, Fennoscandia? The question is all the more intriguing because most of the individual areas of plus and minus anomaly in Fennoscandia, central Europe, and the United States (east of the 110th meridian), when based on the International formula, seem not to be so broad as to forbid belief in the competency of the lithosphere, unaided, to carry the corresponding loads. As a possible route to a solution of the problem we shall inquire as to the effect of changing the spheroid of reference.

The officers of the Survey of India have proved that their measurements of gravity cannot be made to fit satisfactorily the International formula for the earth's figure and have tried a number of other formulas. One, bearing the name "Spheroid of India II," is now preferred by the Survey; it is entered in Table 3. When this special formula is used for

TABLE 3.—*Formulas for the shape of the smoothed geoid*

ϕ = latitude; λ = longitude (positive when east of Greenwich meridian)

Computed by:	
Cassinis (International)	$\gamma_0 = 978.049\,(1 + 0.0052884 \sin^2 \phi - 0.0000059 \sin^2 2\phi)$
Helmert, 1901	$\gamma_0 = 978.030\,(1 + 0.005302 \sin^2 \phi - 0.000007 \sin^2 2\phi)$
Survey of India (II)	$\gamma_0 = 978.025\,(1 + 0.005234 \sin^2 \phi - 0.000006 \sin^2 2\phi)$
Heiskanen, 1938	$\gamma_0 = 978.0524\,[1 + 0.005297 \sin^2 \phi - 0.0000059 \sin^2 2\phi + 0.0000276 \cos^2 \phi \cos 2\,(\lambda + 25°)]$

reduction, it is found that the large area of negative anomaly shown in Figure 8 becomes broken up into three areas, roughly equal to the positive and negative areas on the map of geoidal contours (Fig. 4).

An essentially similar result accrues when Heiskanen's (1938b) "triaxial" formula (Table 3) for the earth's figure is made the basis of reduction. It involves a longitude term and an elliptical equator. The long axis of the equatorial ellipse emerges at 25° West Longitude (angular distance west of the Greenwich meridian reckoned as negative) and at the antipodal point. The short axis emerges at 65° East Longitude and the antipodal point. Heiskanen found the difference of the semi-axes to be about 352 meters, or considerably more than Hirvonen (1934, p. 69) had found from fewer data.

If Heiskanen's formula truly represents the earth's field of gravity, the corresponding anomalies can be found from the Hayford anomalies (computed by the International formula) by adding the appropriate quantities. For example, the Hayford anomaly at a point on the equator and at 25°

West Longitude, where the term involving latitude and longitude dominates, should be reduced algebraically by about 31 milligals. The Hayford anomaly at the equator and at 65° East Longitude should be increased algebraically by 24 milligals. Considering both latitude (averaging about

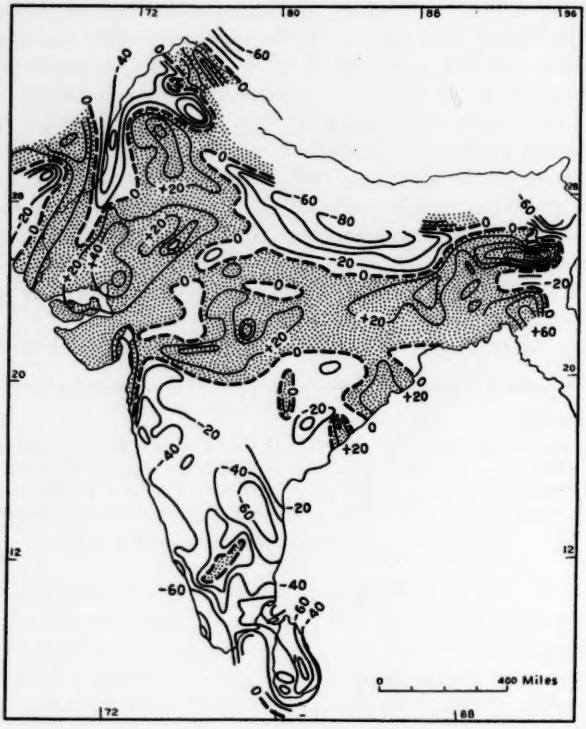

FIGURE 9.—*Map of Hayford anomalies in India, based on Helmert's 1901 formula*

Positive areas indicated by stippling.

20°) and longitude (averaging about +77°), the algebraic increase for the Indian anomalies would average 17 milligals approximately; however, the required correction would be somewhat more than that at the southern end of the peninsula and somewhat less over the Indo-Gangetic plain. When the corrections are made and the isanomaly lines plotted, the resulting map is closely similar to that showing the Hayford anomalies when referred to the Helmert 1901 spheroid of reference (with formula in Table

3). Such a map has been drawn by the officers of the Survey of India and copied as Figure 9.[4] Here we see the Hidden Range emerge as a positive belt (stippled), the gravity troughs to north and south being less negative than in Figure 8, a map based on the International formula.

If Heiskanen's triaxial formula is approximately correct, the difficulty of correlating India with the rest of the earth's gravity field is much diminished. The relative gravitational contrast of the Hidden Range with each of the adjacent troughs remains little changed, but the mechanical problem of the gravity field becomes less serious.

In the first place, every Hayford anomaly calculated in the whole peninsular area is increased algebraically about 17 milligals over the value found when referred to the International formula; by so much is there reduction of the upward pressure expected because of the negative character of an anomalous area. Where about 80 per cent of the region was negative with the International formula, now only about 50 per cent has that sign; instead of being almost continuously negative from Cape Comorin to the Himalayas, the anomaly field of the peninsula has become triple, and the span of each of the unbalanced areas is much smaller than the span of the dominantly negative field deduced from the International formula.

If the alternating negative and positive areas correspond to loads borne steadily and only by the lithosphere, this earth-shell must be able to withstand notable stress-difference. Because of the irregular ground-plans of the three areas, that degree of strength is not easy to estimate. However, the strength must be considerably less than that involved if the three areas were parts of a spherical harmonic—that is, were segments of two negative and one positive belts running all the way around the globe. For such harmonic loading of a floating lithosphere Jeffreys (1932, p. 69) has derived an expression for the maximum stress-difference within the shell. He finds it to be $ghld/\pi t$, where g is the acceleration of gravity, approximately 1000 dynes/cm^2; h is half the height of the harmonic load; l is the wave length of the harmonic; d is the density of crust-rock, taken as 2.7; and t is the thickness of the lithosphere, taken at 60 kilometers.

In the present case h is about 200 meters, and l is about 1200 kilometers. The maximum stress-difference comes out as approximately 340 kg/cm^2. Since the three areas of the gravity field are limited in length (measured along crest and trough axes), the actual stress-difference should be much less than 340 kg/cm^2 and thus not obviously beyond the strength of the lithosphere. This conclusion is the more justified

[4] Chart 12 of the Survey Report for 1936 and Chart 10 in the Report for 1937.

if it be true that pressure more than counterbalances the weakening due to increase of temperature with increase of depth.

The writer is not certain whether Jeffreys' analysis takes full account of the supporting power of the lithosphere derived from its curved form. It may be recalled that Hoskins (1906, p. 581) found, by computation, that the lithosphere, if somewhat stronger than the best granite, can support about one five-hundredth of its own weight, merely because of the "domed" form of the shell. Does this principle furnish additional evidence that the Hidden Range load can be stably borne by the lithosphere alone?

Further, according to the map (Fig. 9), the broad positive belt is interrupted by two negative areas and by several long, bay-like, negative re-entrants. All these negative areas mean excess of upward pressure by the asthenosphere, upward pressure that locally offsets the tendency of the belt as a whole to sink. The negative segments of the belt may be called "supporting pillars" for the Hidden Range part of the lithosphere.

We have already noted that part of the regional negative anomaly of the northern area can be explained by the low density of the Indo-Gangetic alluvium. But, even if all of the regional anomaly represents a negative load, the stresses within the lithosphere under this surface of comparatively small span do not seem too great to be borne.

The maximum and average anomaly in the southern gravity trough are each about 20 milligals less negative than the maximum and average anomaly in the northern trough. (*See* Figure 9.) On the other hand, the span of the southern negative area is much greater than the span of the northern. It is therefore less easy to credit the lithosphere under the former surface with sufficient bending strength to bear the stresses involved. The difficulty is partly relieved when account is taken of the three small positive areas interrupting the southern field. Each of these positive areas represents what may be called "sinkers"—that is, excess weights that help to offset the excess of upward pressure by the asthenospheric material. Nevertheless, considerable stress must reside in the lithosphere, if the map of re-computed Hayford anomalies, given in Figure 9, corresponds to the truth in principle.

Glennie (1938), recognizing that the Hayford anomalies are based on the manifestly incorrect assumption of local isostatic compensation, has calculated what he calls the "normal warp-anomalies" for India. They were calculated after making two assumptions: First, that the topography out to the radial distance of 22.5 miles or 120,000 feet from each gravity station is not compensated; second, that the spheroid of reference should be the figure described by the Spheroid of India II formula. This formula gives for India a geoidal surface which differs but little from that belong-

ing to Heiskanen's triaxial figure. The isanomaly lines showing the distribution of Glennie's normal warp-anomalies are drawn on the map of Figure 10, where the stippling shows at a glance the considerable increase of the positive (Hidden Range) belt over the extent portrayed in Figure 9.

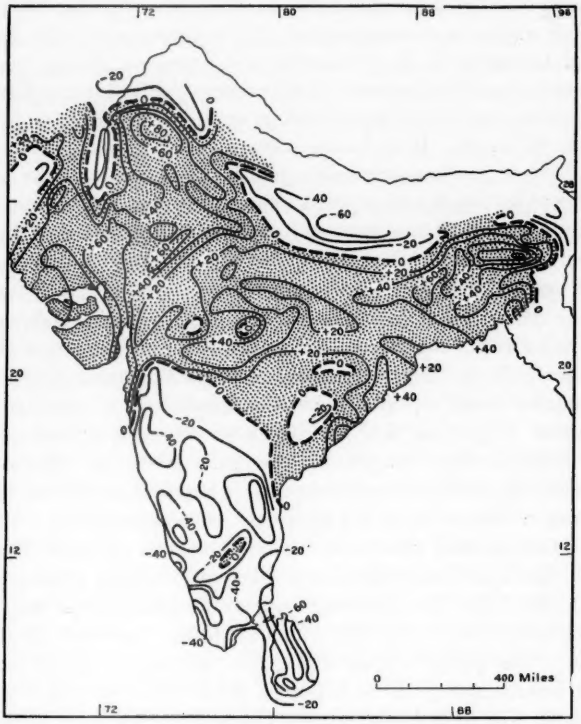

FIGURE 10.—*Map of Hayford anomalies in India, based on the "Survey of India II" formula for the earth's figure and on Glennie's assumption of regional isostatic compensation*

Positive areas indicated by stippling.

On the average each anomaly in the southern trough area has become about 10 milligals less negative, and the span of this area has been diminished. Thus the implied negative load on the lithosphere under this surface and the corresponding stresses have been considerably decreased by the assumption of regional compensation. On the other hand, the positive load on the lithosphere under the Hidden Range area has

become greater, and the lithospheric stresses greater. Here again a question arises: Are the stresses too taxing to be borne by the lithosphere, which has the "domed" form as well as intrinsic strength?

Glennie's explanation of these wide fields of anomaly may be summarized. He accepts Jeffreys' conclusion regarding the outer earth-shells, as stated in the second edition of *The Earth* (Jeffreys, 1929). Accordingly the Sial is supposed to be composed of a granitic layer with density of 2.67 and thickness of 10 kilometers, and a second, deeper layer with density of 2.85 and thickness of 20 kilometers. This "intermediate" layer rests on Sima, assumed to be crystalline dunite with density of 3.3 and of indefinite thickness. It is further assumed that the Indian segment of the lithosphere was long ago deformed, the middle part of the peninsula being upwarped and the broad belts to north and south being downwarped. The axes of warping are supposed parallel to that of the Himalayan chain of mountains. After the upwarping the middle belt was peneplained but retained an excess of mass because both of the two lower, relatively dense layers had been pushed up; hence the Hidden Range. On the other hand, the original thickness of the downwarped, light Sial has been wholly or largely retained. Thus, within the respective limits of the northern and southern belts, the denser rock of the intermediate and dunitic layers had been carried down an extra distance from the present surface where the gravity pendulum is swung. Hence both of these belts show defect of mass and negative Hayford anomalies. Glennie's conception is illustrated in the lower section of Figure 5.

While the assumed density of the third layer is probably 10 per cent too high, the idea of warping or, better, fracture-warping gives in principle a reasonable basis for understanding the Hidden Range belt and the northern belt, occupied by the sediments filling the Indo-Gangetic geosyncline. The southern area of negative anomaly may be the site of ancient downwarping, but it bears no thick prism of young sediments comparable with that in the north. Hence Glennie's warp hypothesis is not so directly supported by the facts of field geology when the southern area is under discussion. An alternative or supplementary explanation of its dominantly negative anomaly seems worth study.

This southern part of peninsular India is comparatively narrow, bordered by the ocean on three sides, and the site of prolonged erosion. The strength of the lithosphere has prevented complete isostatic adjustment for this secular denudation. To east, west, and south the stiff lithosphere has refused to yield to the full extent needed for the restoration of isostasy; here there is a threefold exemplification of what may be called a "coast effect"—a general principle to be described on a later page. Is the

regional negative anomaly of southern India to be in large part accounted for by this principle?

ATTEMPT AT A RECONCILIATION

While the theoretical deductions regarding the distribution, kinds, and intensities of the loads on the Indian segments of the lithosphere somewhat ease the problem of the elastic support of those loads, it must be fully recognized that the lithosphere here needs to have more strength than that accepted by many isostasists. This conclusion is now to be faced with the fact that the lithospheric segments of Fennoscandia and northeastern North America are actually yielding under asthenospheric pressure of intensity much like that deduced for the Indian segment. That there seems to be no necessary conflict may become clearer when allowance is made for certain contrasts of the conditions ruling in either of the glaciated tracts and in India.

The reconciliation is not likely to be founded on the existence of low density possessed by the thick Indo-Gangetic alluvium on the north, nor on the assumption that great, high-level injections of Deccan trap, with relatively high density, underlie the Hidden Range area. Theoretically such distribution of densities at or close to the surface of the peninsula, even in isostasy, would give a warped geoid. There would be a gravity trough on the north and gravity crest in mid-peninsula; but this geoidal warping could hardly be more than a small fraction of the observed warping, and this double hypothesis would leave unexplained the southern area of negative anomaly. Again, the speculative idea, that the strong earthquakes of north India mean isostatic adjustment actually in progress along the northern belt of negative anomaly, does not solve the problem of the enduring Hidden Range and the southern area of enduring negative anomaly. In any case it looks as if the regional departures from isostasy are real and, in large part at least, represent "permanent" stress within the lithosphere.

Two other conditions, of greatly differing importance, suggest how the actual stability of India may be reconciled with the mobility of the glaciated tracts under lithospheric loads of comparable intensity.

First, the recent and apparently continuing deformation of the young sedimentary formations of northern India indicates that there may be special horizontal compression of the lithosphere throughout the peninsula, a compression directed across the axes of the Hidden Range and negative belts. Such tangential compression should here increase the strength of the lithosphere, other things being equal, above that expected under the surface of Fennoscandia or eastern Canada, where there is no evidence of tangential compression in abnormal amount.

But the reconciliation is much more likely to center in the other of the two conditions. The stiffness of a circular plate, loaded as in all the cases here considered, varies inversely with the second power of the radius of the plate, to a close approximation. Now the radius of either of the glaciated tracts is two to five times the radius of any of the areas of one-sign anomaly in India (Heiskanen's triaxial figure taken as the basis for calculation). The respective bending moments have different orders of magnitude, and, if the lithosphere in India is anywhere near the yielding-point under the loads suggested by the gravity anomalies, then we may be sure that Fennoscandia will long continue to rise isostatically.

It may also be observed that the Fennoscandian and Canadian loads are negative and therefore without the special support that comes from the domed form of the lithosphere, while, on the contrary, the greatest deduced load in India is positive and has that special support (Hoskins principle).

To summarize: The evidence for isostasy, supplied by geologists and hydrographers in the glaciated tracts as well as by the geophysically minded geodesists who have measured gravity over wide stretches of North America and Europe, appears not to be necessarily refuted by the results of gravitational studies in India. But the observations by officers of the Survey of India do show, more clearly than ever, that isostatic compensation for topography is regional, and that the departures of India from isostatic equilibrium are less drastic if the departures are judged by the anomalies referred to Heiskanen's triaxial figure of the geoid rather than by the anomalies referred to the International formula. It is therefore expedient to review a few of the salient reasons why geologists should reflect on the possibility, if not probability, that the sealevel equator is not a circle but approximately an ellipse. This is an idea to which geodesists have had to recur ever since the year 1861, when A. R. Clarke announced it, new accumulations of data prompting fresh attack on the problem of the best figure for the earth.

(1) Heiskanen's formula is to be regarded with particular respect because it is founded on more facts of observation than those used in previous investigations, whether by Clarke, Helmert, Berroth, Hirvonen, or Heiskanen himself, and because Heiskanen's method of carrying through this delicate enterprise appears to be the best method yet devised.

(2) His formula offers a good basis for explaining some broad, meridional belts of one-sign anomaly—belts derived when the International formula with circular equator is used.[5] The belts are so wide that, if they

[5] The triaxial formula computed by R. A. Hirvonen (1934, p. 69) has emergence of the long axis of the equatorial ellipse at 19° West Longitude (−19°), and thus only 6° from the position given by Heiskanen's 1938 formula (−25°). The less adequately documented formulas of Helmert (1915), Berroth (1916), Heiskanen (1924), and Heiskanen (1928) made the long axis emerge at +17°, +10°, −18°, and 0° respectively. Except for Heiskanen's 1924 formula, none of these four corresponds well with the observed fact of strong negative anomaly in the Arabian Sea-India region.

were to represent reality, we should have to credit the asthenosphere with considerable strength. On the other hand, according to the newer, triaxial formula, the broad belts of strain have much smaller spans and also smaller intensities of stress.

Gravity on any parallel of latitude should be at minimum along the meridional plane passing through 65° East Longitude, and we have already noted the needed change of the old Hayford anomalies to correspond with the change in the spheroid of reference. The result of such change is to make a decided diminution of the field of negative anomaly in India. The same effect is wrought on the large negative anomalies found by Vening Meinesz in the Arabian Sea.[6]

Another broad belt of negative anomaly follows the Pacific coast of the United States. The belt is crossed by the meridian of 120° West Longitude, which is only 5° from that where the short axis of the Heiskanen spheroid emerges on that side of the earth. When computed by the International formula, the gravity anomalies of the great belt are dominantly negative, the averages (51 stations) being:

Kind of anomaly	Size (milligals)
Hayford (depth of compensation 113.7 km.)	−28
Hayford (depth of compensation 96 km.)	−26
Hayford (depth of compensation 56.9 km.)	−17
Airy (sealevel thickness of Sial 40 km.)	−22
Heiskanen-Airy (sealevel thickness of Sial 50 km.)	−21

These averages would be decreased numerically 6 to 12 milligals if computed by Heiskanen's triaxial formula—that is, they would become less negative by the amounts stated. Moreover, the belt showing negative anomaly after the change would be only about half as wide as the field of negative anomaly derived by use of the International formula. Hence California and the adjoining coastal region would be a region easier to understand if the principle of (regional) isostasy is true.

Again, the large positive anomalies found by Vening Meinesz over the eastern Atlantic would be much diminished by replacing the International

[6] Similarly the mechanical problem represented by the wide Ferghana field of the Pamirs becomes somewhat more tractable. According to V. Erola (1938) this field is many hundreds of kilometers in width and shows relatively enormous Hayford and Airy anomalies when based on the International formula. Their values would be increased algebraically only about 6 milligals by changing the spheroid of reference to Heiskanen's 1938 formula; the mean isostatic anomaly over this vast area would still be nearly −50 milligals. Is it possible that the centrospheric inequality, hypothetically assumed to account for the negative field of peninsular India, is accentuated as it continues to the northward?

Again, does the seismicity of the Pamirs indicate actual yielding of the lithosphere, so that in the future isostasy will become more nearly realized?

Finally, much depends on the accuracy of the field measurements in this difficult terrain. Large error has been discovered, as for example at the Kalai-Wamar station, where the prodigious anomaly of −358 milligals has been reduced to −150 milligals by a more recent measurement of gravity.

All these possibilities should be considered before the Pamirs data can be taken to prove notable strength in the asthenosphere.

formula for the figure of the earth by Heiskanen's triaxial formula. The decrease would be 31 milligals at the equator and at 25° West Longitude, where the long axis of the equatorial ellipse emerges, and 21 milligals at 45° North Latitude on the same meridian. In western Scotland, between the fifth and seventh meridians of West Longitude, 20 stations gave Bullard and Jolly (1936, p. 473) a mean Hayford anomaly of +14 milligals (International formula, 113.7 kilometers). This mean would become nearly zero if computed by the Heiskanen formula.

The East Indies, on the other side of the globe, are 25 to 45 degrees of longitude from the point where the long axis of the ellipse emerges and are close to the equator. The mean Hayford anomaly for a couple of hundred stations at sea gave Vening Meinesz a value of +20 milligals when based on the International formula. The mean would be reduced from 4° to 13° if computed by the triaxial formula. It is also possible that a further reduction would be necessary if the Hayford anomalies for many land stations in the archipelago were determined.

Is it an accident that the new formula helps to reconcile the principle of isostasy with the five wide regions that show large one-sign anomaly when reduction is based on the older International formula? For none of these regions is the mean anomaly reduced to zero, but in general each remaining anomaly may well be still further decreased when more gravity stations shall have been occupied and if the pure Pratt-Hayford hypothesis of isostatic compensation is replaced by the much more probable hypothesis that the compensation really follows laws involving the ideas of both Pratt-Hayford and Airy. Such a change of base better satisfies the geologist, and Heiskanen (1932; 1938a) has shown that the combination of ideas has merit from the geophysical point of view. Elsewhere the present writer (Daly, 1938a, p. 191) has raised the question whether the conception of "anti-roots" may not also have to be entertained before the complex problem of isostatic compensation is adequately solved. Probably the anomalies computed on this triple basis would in many cases not differ much from those found with the Airy hypothesis (sealevel thickness of the Sial at 35 kilometers) or with Heiskanen's modification of the Airy hypothesis.

RELATION OF EARTH'S ASSUMED TRIAXIALITY TO DISTRIBUTION OF STRESS

Jeffreys (1929, p. 222) has made clear that a permanently triaxial earth with dimensions like those calculated by Heiskanen must be able to withstand stress-difference which in maximum should reach several tens of atmospheres. Since the loads approach or reach hemispherical spans,

the maximum shearing stress must be at the depth of thousands of kilometers, and a lithosphere less than 50 miles or 80 kilometers in thickness could not bear anything like the whole of the stress involved. If, then, we accept as true the doctrine of isostasy, meaning thereby utter weakness of the asthenosphere a few hundreds of kilometers thick, nearly all of the stress associated with the triaxiality would reside in a thick centrospheric shell beneath the asthenosphere. Thus we have arrived at the suggestion made by that far-seeing geologist, G. K. Gilbert (1913, p. 35), who asked whether broad fields of one-sign anomaly of gravity may be largely explained by centrospheric inequalities of mass.

SPECULATION REGARDING A CAUSE FOR TRIAXIALITY

Such an arrangement of the centrospheric material could be understood, if this material is crystalline and therefore strong and also of density somewhat higher than the density of the asthenosphere. Of course the Heiskanen formula, like the International formula, represents generalization of a surface that is irregular and wavy in detail but, to secure Heiskanen's mean geoidal figure, there must be a relative excess of mass at or near the equatorial section and extending over a large part of a hemisphere, with perhaps another excess of mass in the antipodal region. The suggested increase of density in passing from asthenosphere to centrosphere would be natural if the asthenosphere is vitreous, while the centrospheric layer just beneath it is chemically similar but crystalline and therefore denser. Under this condition an unequal distribution of mass would result if in at least one hemispherical sector there is a broad "hump" on the centrospheric layer.

The described degree of asymmetry for the centrosphere might be theoretically regarded as an inheritance from the cosmogonic stage of our planet's history or as the effect of differential raidoactivity of the Sial-mantled and Sial-free sectors. These speculative ideas will not here be elaborated. Elsewhere the writer (Daly, 1938b, p. 420) has given them brief expression and has raised the question whether the contact between asthenosphere and centrosphere occurs at the Jeffreys-Bullen discontinuity located about 475 kilometers below the earth's surface.[7] Naturally such guesses cannot now be checked by facts of observation and can rate no better than as working hypotheses. More assured is the view that neither seismology nor geophysics in general seems to forbid the possibility of large-scale departure of the centrosphere from perfect circularity in east-west sections. The gravitational attraction of the

[7] This discontinuity is apparently the same as that placed at the depth of about 400 kilometers by S. Mohorovičić (1925) and by P. Byerly (1926).

unevenly distributed centrospheric matter would pile the material of a thoroughly weak asthenosphere over the "humps," having removed this material from the corresponding "hollows" in the centrospheric figure. All three layers of the earth's body—asthenosphere, lithosphere, and ocean—would be brought into approximate conformity with the hump-and-hollow surface of the centrospheric shell. Similar departure of equator and parallels of latitude from circularity would result if the centrospheric shell bears a number of "humps," concentrated in an equatorial sector on one side of the globe.

This hypothesis of an asymmetric, crystallized, strong centrospheric shell of great thickness is offered for the criticism of geophysicists.

GRAVITY ANOMALIES OVER THE PACIFIC AND ATLANTIC OCEANS

From the mean of the gravity anomalies originally reported by Vening Meinesz (1934, p. 104) for the open oceans beyond the continental shelves and far from localized Deeps, he concluded that there is excess mass within the huge earth-sectors covered by the Pacific and Atlantic oceans. Four years later B. C. Browne (1937, p. 271) showed that many of the anomalies were affected by an error, owing to the motion of the submarine vessel in which the gravity measurements were made. By actual tests Vening Meinesz (1938) proved the reality of this error and published a table of corrections for the "Browne term." From that record the present writer has computed the corrected anomalies, with the result shown in Table 4.

TABLE 4.—*Average gravity anomalies, 36 stations, open ocean, far from "Deeps"*
(International formula, milligals)

	Free air	Hayford	Airy-Heiskanen	Regional (Vening Meinesz)
Without the Browne term...	+8	+13	+8	+12
With the Browne term.....	+1.8	+ 7	+2	+ 6

The comparatively small values of the corrected anomalies do not indicate evident departure of these vast regions from practically perfect isostasy. Moreover, the mean anomalies may well be further diminished with increase of the number of stations occupied and perhaps also with improvement of the reference figure of the earth.

GRAVITY PROFILES OFF CONTINENTAL SHORES

Vening Meinesz (1934, p. 99, 206) has shown that there is an apparently general increase of the isostatic anomalies as the ocean deepens from continental shores to moderate distances beyond the feet of the continental slopes, the same tendency being manifest off the beach-lines of the larger islands. Table 5 illustrates the case, and Heiskanen (1938a, p. 13) has listed a score of other instances also reported by Vening

TABLE 5.—*Isostatic anomalies off shores*

(International formula, milligals)

Section	Station	Approximate length of section (km.)	Depth of water (m.)	Hayford anomaly	Heiskanen-Airy anomaly	Regional anomaly (Vening Meinesz)
Off San Francisco....	98	75	680	−57	−53	−63
	99	.....	3760	+27	+ 8	+29
Difference.....	.....	.....	.....	84	61	92
Off southern Mexico..	86	65	3050	−30	−37	−26
	85	.....	5030	+23	0	+36
Difference.....	.....	.....	.....	53	37	62
Off Brittany (a).....	426	350	ca. 190	−34	−26	−35
	427	.....	3650	− 9	−19	− 9
Difference.....	.....	.....	.....	43	45	44
Off Brittany (b).....	486	375	ca. 190	− 7	0	−20
	485	.....	4600	+47	+23	+25
Difference.....	.....	.....	.....	54	23	45
Off Sokotra.....	17	400	80	−61	−51	−73
	18	.....	4210	− 3	− 8	− 9
Difference.....	.....	.....	.....	64	59	82
Off Ceylon.....	25	100	65	−52	−50	−62
	26	.....	3920	−19	−27	− 3
Difference.....	.....	.....	.....	71	77	65

Meinesz. The average algebraic increase of the Hayford anomalies is about 65 milligals. Vening Meinesz remarks on the "unexpected" character of this rule, which "means over-compensation of the coastal topography." He briefly discusses hypothetical explanations of the rule: first,

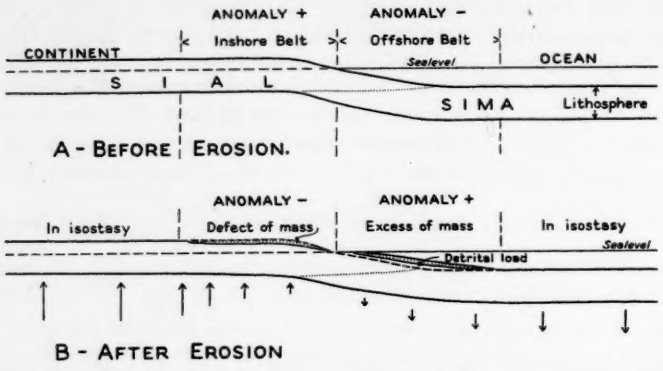

FIGURE 11.—*Sections illustrating an explanation for regional departure from isostasy along continental shores where erosion has been prolonged*

the influence of descending convection currents, and, second, "a tectonic folding phenomenon."

The present writer offers a third explanation, in terms of the "coast effect" already noted. Prolonged erosion of continent or large island removes load from the corresponding earth-sector and adds some load to the adjacent "shelf" sector. Because the lithosphere has considerable bending strength, a dry-land belt along the coast cannot rise to the full amount needed to restore complete isostasy. Incidentally this deduction seems to account well for the coast-belts of negative anomaly on the Atlantic and Pacific borders of the United States. On the other hand, an offshore belt paralleling the shore zone cannot, because of crustal strength, sink to the full amount demanded if isostatic adjustment for the mass of new shelf sediments is to be complete. (*See* Figure 11.) Accordingly Table 5 illustrates the tendency for the isostatically reduced anomalies to be positive for a moderate distance from the shore zone of each continent and large island, while in any case there is algebraic increase of the anomalies as deeper water is approached.

GRAVITY OVER THE OCEAN "DEEPS"

Hecker, the pioneer in the measurement of gravity at sea, found the Tonga Deep to be a belt of great negative anomaly. Since then, similar

defect of mass on a large scale has been proved in general to characterize such long, narrow troughs that vary the floor of the deep ocean. The defect of mass is reflected not only in the Free-air anomalies but also in the anomalies remaining after any of the recognized types of isostatic

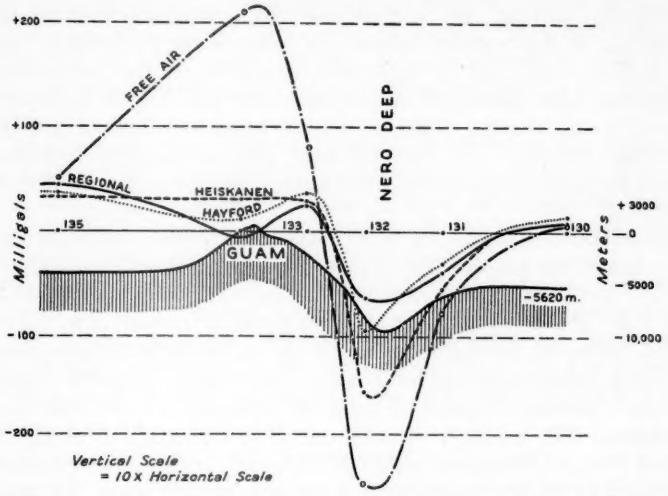

FIGURE 12.—*Gravity anomalies along a section across the Nero Deep*
After F. A. Vening Meinesz.

reduction. These facts are illustrated in Figure 12. If the Japan (Nippon, Tuscarora) Deep is a stable inequality of the submarine topography, its span and the magnitude of its negative anomalies, as reported by Matuyama (1934), appear to show the lithosphere under the Pacific to be at least twice as strong as in the continental segments. The same may be said of the Nero, Mindanao, Nares, and other Deeps.

GRAVITY OVER THE MEDITERRANEAN BASINS

Finally, the conception of little or no strength in an asthenosphere that begins at depth no greater than 80 kilometers is to be confronted with the values of gravity over the mediterranean seas of Europe, the West Indies (including the Gulf of Mexico), and the East Indies. Here the maximum isostatic anomalies (International formula) reach values between $+50$ and $+100$ milligals, average anomalies as reported for individual basins approaching $+20$ to $+50$ milligals, and diameters for the basins being 300 to 600 kilometers or more. If the new Heiskanen

formula for the geoidal figure is correct in principle, the mean anomalies of some of the basins should be algebraically reduced, but these would still be strongly positive.

Vening Meinesz (1934, p. 98, 198, 203) tentatively explains the sectors of excess mass by assuming that each basin is the site of a descending convectional current. He is doubtless fully aware that his idea is a hypothesis *ad hoc,* without independent evidence for its validity. Is it out of the question that the rock loads responsible for the positive fields of anomaly here considered can be carried by the unaided lithosphere, some of whose strength is due to the curved, domed form of this shell (Hoskins principle)? On the other hand, may not the positive fields be in part explained by horizontal inequalities of mass beneath the asthenosphere?

These questions are hardly to be answered until more is known about the origin of the basins. The reason for the great depth of the Gulf of Mexico, for example, is a mystery. If the water and also the 10,000 meters of light sediments along the Gulf Coast are underlain by the Sial in full thickness, an understanding of the relatively close approach of the whole region to isostasy is not easy.[8] It would be necessary to postulate an abnormal thickness of specially dense Sima below the Sial of the region. This condition would be met if in comparatively recent geological time the lithosphere under the Gulf and its shore was abnormally thickened by the crystallization of a vitreous, basaltic layer. Or, applying Barrell's (1927) general idea, we might imagine gigantic, sill-like injections of heavy, ultrabasic material into the lithosphere.

Both hypotheses are troubled with difficulties. The second could hardly be entertained at all unless we suppose also that the basaltic part of the vitreous substratum is relatively thin and rests on ultrabasic, vitreous material which is not too deep to be so injected. As a matter of fact there is some reason to doubt that the bottom of the basaltic layer is more than about 100 kilometers below the earth's surface.

Each of the conceived processes for increasing the density of the deeper part of the lithosphere involves drastic dislocation of this shell. It is therefore significant that basins like those of the Gulf of Mexico, the Caribbean Sea, the East Indies, the Sea of Japan, Okhotz Sea, and the Mediterranean Sea of Europe are related geographically and structurally to respective belts of intense Cenozoic orogeny. This relation suggests syngenesis of basin and mountain chain. Were the profound, orogenic disturbances of the Sial accompanied by wholesale stoping of the deeper and denser lithospheric rock, with consequent regional chilling and crystallization of the vitreous substratum to extraordinary depth? Is it

[8] The mean Hayford anomaly for the 17 stations occupied is +16 milligals (International formula).

possible that such complex displacement of the deeper material of the lithosphere led to the tapping of the underlying ultrabasic layer?

If, by either process or by both processes, the mass in a unit column of the sub-Sialic part of the lithosphere has been increased under a sea basin, the isostatic compensation in the same sector should be larger than that computed on the standard assumptions, and there would be decrease in the absolute values of the isostatic anomalies. Thus any rock load implied by the new regional anomaly would be correspondingly smaller than the load implied by the published Hayford anomaly for the same region.

The alternative hypothesis—that the Sial is absent or very thin under the Gulf of Mexico and similar basins—must also remain in the realm of conjecture until testing by seismological or other methods becomes possible. In the meantime we may well doubt that the Sial lacks normal thickness under the sediments of the Gulf Coast.

Where there has to be so much guessing, it is clear that the problem of origin for the sea basins is highly obscure, so that the true meaning of the positive anomalies over their surfaces cannot now be declared. Nevertheless, those anomalies do not seem to compel belief in a vertical distribution of strength greatly different from that indicated in continental regions.

SUMMARY

It is now generally agreed that each continental segment of the lithosphere is in almost perfect balance with each segment that is covered by any of the oceans. There is also general agreement: (1) That isostatic compensation is regional, not local—a fact necessarily implying permanent stress-difference in, and strength of, the lithosphere; (2) that the average thickness of the lithosphere is not much more than 100 kilometers; and (3) that the lithosphere is underlain by an asthenosphere. Less unanimous are the views of geophysicists and geologists regarding: (1) The strength of the asthenosphere and corresponding maximum for stress-difference within it; (2) the thickness of the asthenosphere; (3) the true geoidal figure of the earth; and (4) the degree of parallelism between the geoid and the levels—equipotential surfaces—below the surface of the globe.

The observed facts recounted in this paper suggest: (1) That the asthenospheric material must flow under a shearing stress less than 3 kilograms per square centimeter; (2) that the major belts of one-sign gravity anomaly on land and at sea are apparently not to be satisfactorily correlated with such asthenospheric weakness, if the International formula for the figure of the earth be assumed when computing

the anomalies; (3) that the 1938 triaxial figure of Heiskanen is helpful in providing a basis for the reconciliation; (4) that, if this be true, the stresses associated with the departure of the geoidal equator and parallels of latitude from circularity must be largely borne by a thick, centrospheric layer beneath the asthenosphere; (5) that there is no evident reason why the geoidal figure should not approximate a triaxial ellipsoid; (6) that the algebraic increase of the isostatic anomalies with increase of depth for the ocean offshore from continents and large islands seems referable to a systematic form of regional compensation, owing to the failure of the strong lithosphere to attain fully the isostatic equilibrium which had been disturbed by prolonged denudation of the lands; (7) that the ocean Deeps, like some of the smaller glaciated tracts, give an idea of the minimum strength of the lithosphere; and (8) that the areas of positive anomaly represented in the mediterranean seas appear to indicate at least as high a degree of strength for the curved, domed, lithospheric shell of the earth.

The foregoing suggestions are of course tentative. Their statement does not mean that the writer believes them throughout to represent the truth. His main object is to emphasize once more a fundamental and ineluctable problem of earth science and to ask those more competent in geophysics to attack the problem in the light of the new facts discovered by Hirvonen, Heiskanen, Erola, Hunter, Glennie, Vening Meinesz, Cassinis, Matuyama, and the experts participating in the measurement of gravity in the West Indian region. Of those problems not the least baffling is the contrast of behavior of the Basement Complex in India and in Fennoscandia as this Sialic layer feels pressures of comparable intensity.

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DEFORMATION OF THE EARTH'S CRUST

BY WALTER H. BUCHER

(Anniversary Day Address)

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INTRODUCTION

If the deformation of the earth's crust, to which the face of the earth owes its salient features, had been one dynamic event, a careful analysis of the pattern produced would yield sufficient clues to lead directly by inductive reasoning to an understanding of the forces that produced it. But the features that we see are the result of innumerable acts of deformation, each later one more or less superimposed upon and partly obliterating the patterns produced by the earlier ones. The unraveling of the superimposed patterns would be a baffling task even if the whole face of the earth were exposed to view and if our knowledge of it were complete. With less than one-third of the face of the earth accessible to view and so very little of that known accurately, we can only base our reasoning on generalizations we believe to be fact, giving weight to different groups of facts depending on the limits of our personal experience. The result is that we hold a multitude of conflicting views concerning the forces that deform the earth's crust. This is not a discredit to our science. It is an

inevitable stage in the growth of knowledge in any field of great complexity, in which the formulation of individual hypotheses is fruitful in the extent to which we take care to set forth carefully what we consider to be facts, and what conclusions we believe can be drawn from them, and, when necessary, stand ready to revise with our foundation of fact the structure of thought we have built on it. It is in this spirit of give and take that I shall set forth what I personally consider to be fundamental facts and what conclusions I feel compelled to draw from them.

THE GREAT CYCLE

GEOMORPHOLOGICAL EVIDENCE

In all continents the present topography shows two striking features: (1) Abundant evidence that in the not distant geologic past there were no high mountains and most of the land surface was worn down to near sea level—*i.e.*, more or less peneplained; and (2) equally abundant evidence that in the most recent past all lands, even the vast lowlands of western Siberia and those east of the Andes of South America, have undergone rejuvenation. This rejuvenation is of importance in two ways: First—It, like the earlier state of far-reaching peneplanation, is world-wide; the change has affected all parts of the earth essentially at the same time. Second—It has progressed at an accelerated pace.[1] In mountains as well as in plateau lands, deep, steep-sided valleys are incised in the floors of older, broader, shallower valleys. Clearly the earth's surface is approaching a condition of maximum relief, having passed in the not distant past through an opposite condition of minimum relief.

STRATIGRAPHIC EVIDENCE

That these two conditions represent extremes in a cyclic process that swings back and forth between minima and maxima is shown by the stratigraphic record, the only record we have of earlier conditions on our planet.

The more the stratigraphy of the continents is studied comparatively, the more evident it becomes that times when large parts of the continents were covered by the sea have alternated with times when most of the continental areas stood above sea level. This great rhythm was first clearly recognized by Eduard Suess (1888); it was accepted as the basis of stratigraphy by T. C. Chamberlin (1898); Joly has described it graphically; recently Grabau (1934; 1936) has made it the basis of his "pulsation theory." It can be nothing less than the expression of a great pulse

[1] For the Susquehanna River, for instance, Campbell (1933, p. 572) has computed the ratios of rock volumes that had to be removed to form the successively incised valleys as: 30: 9: 7: 1. For other examples *see* Bucher (1933, p. 394-402); Fisk (1938, p. 71).

in the earth's crust that in a broad rhythm increases and decreases the relative distance from the ocean floors to the continental levels.[2]

BASINS AND SWELLS

Several basic facts are revealed by the structure of the earth's crust, insofar as it is accessible to us. On the continents we recognize an important datum plane—the surface which separates the pre-Cambrian complex of rocks from all later ones. This datum plane and the overlying mantle of sediments have undergone deformation in two ways: one has produced strongly elongated, essentially linear elements of structure; the other broad, essentially nonlinear features (*See, e.g.*, Moss, 1936).

Let us turn to the nonlinear features first. We may call them "basins" and "swells." Dominating the picture are the basins, such as the Michigan and the Indiana-Illinois coal basins, the Paraná basin of Brazil and the adjoining countries, the Congo basin in Africa. The "swells" form structural highs between the basins, generally of quite irregular outline. The evidence is growing steadily that most of the swells and basins had their origin as far back as late pre-Cambrian or at least early Paleozoic time and that they maintained their negative and positive tendencies over long periods of time.

Even more significant is the fact that nonlinear elements of similar pattern dominate the present surface of the oceans. Here the revolutionary method of echo sounding is rapidly filling the enormous gaps in our knowledge. A study of the new map of the Southern Atlantic which has resulted from the cruise of the *Meteor* a decade ago is illuminating (Pratje, 1928). Great basins, such as the Brazilian and Argentinian basin on the west side, and the Cape Verde, Sierra Leone, Guinea, Angola, and Cape basins on the east side, are separated by swells which exhibit a distinct tendency to trend northeastward. Our own East Coast swings in a broad curve from Southeastern Florida to the North Carolina axis that juts out into the sea; a similar curve follows to the north. The manner in which the lines of positive gravity anomalies follow each curve suggests that these curves mark the west sides of two structural basins that lie largely beneath the sea.

The Mid-Atlantic ridge differs from the ordinary swells in consisting of a number of such swells joined together. We shall come back to it later. The general northeast trend of the submarine swells is continued in the swells between the basins on the west side of Africa, as Krenkel

[2] While Grabau's pulsation theory is derived entirely from stratigraphic data and refers to pulsations of sea level, he said expressly (1934): "Worldwide, simultaneous pulsations of marine advance and retreat must be caused by changes within the ocean basins. These changes are tectonic in the largest sense of the word but are to be distinguished from local foldings of sediments."

(1925), over a decade ago, and lately Hans Cloos (1937) have shown. It looks as if the earth's surface between the belts of linear deformation were divided into a mosaic of irregular basins separated by irregular structural highs. Because of its essential lack of linear character, such a pattern must owe its origin to movements normal to the earth's surface. It suggests the sort of pattern that might result from the struggle for space in a not too heterogeneous crust that must adapt itself to a diminishing surface.

GEOSYNCLINES AND OROGENIC ZONES

GENERAL PATTERN OF OROGENIC ZONES

The linear belts of deformation—the great orogenic zones—are few and run widely separated as essentially continuous, relatively narrow bands across large parts of the face of the earth. As such they cannot be due to local causes. They must be the result of stresses that have affected the earth's crust as a whole.

STRUCTURAL DEVELOPMENT OF OROGENIC ZONES

They all show essentially the same history. They arise from relatively broad belts along which the earth's crust at first sank steadily, receiving essentially conformable sediments in greater thickness and at greater rate than other submerged portions of the earth's crust. These are the "geosynclines" in the broader sense of the term. The earlier geosynclinal phase sooner or later is followed by structural deformation. This at first consists largely of strong folding and thrusting—that is, of evidence of dominantly horizontal stresses. Vertical movement, however, prevails more and more and eventually dominates largely, leaving the erstwhile prism of thick conformable sediments as an intricately folded and thrust-faulted young mountain range.

In general, the geosynclinal phases of quiet sinking coincide with those of continental submergence, while the maxima of orogenic deformation coincide clearly with times of emergence (Bucher, 1933, p. 439-442). This can only mean that the same pulse in the stresses within the earth's crust controls the movements in both the geosynclinal orogenic belts and the large areas that lie between them.

But if both linear and nonlinear elements have arisen from the same distribution of stresses in the earth's crust, it is difficult to see why there should be such a great contrast between the two types of deformation, unless the linear type was localized by the pre-existing geosyncline. That makes sense only if the geosyncline owes its existence to stress conditions different from those which cause the orogenic deformation. We shall return to this thought later.

SUPERFICIAL CHARACTER OF DEFORMATION IN OROGENIC ZONES

When we look closer at the structure of the orogenic belts we recognize another basic fact: Even in the most profoundly disturbed orogenic zones, so small a thickness of rock is involved that it can represent only a small outer fraction of what we must assume, for reasons not here specified, to be the thickness of the earth's crust. The deformation is so obviously superficial that the temptation is great to see in it nothing more than the result of a superficial slipping of the sedimentary mantle under the action of gravity. Arguments have been advanced to make it seem plausible that with time such sliding might take place even with very slight angles of inclination.

The fine topography of submarine valleys, which the intensive work of the United States Coast and Geodetic Survey (Rudé, 1938; Stetson, 1938) has brought to light on the Atlantic slope,[3] where thick sediments dip seaward, is the exact opposite of what should be expected if there were a tendency for the sedimentary mantle to slump off parallel with the contours of the submarine surface under the action of gravity. Even the steep submarine slopes which the Snellius Expedition has studied in such detail in the East Indian Archipelago show no sign of such a tendency (Kuenen, 1935).

If the folding of the outermost skin is, then, due to a deformation of the earth's crust as a whole, this is possible only through a process that shears the skin from the rest of the crust. This would be accomplished if the bulk of the crust along such a zone of orogenic disturbance were to buckle downward.

The relatively narrow, long belts of abnormally high negative gravity anomalies over modern active orogenic zones seem to indicate that such a downward buckling of the deeper crust actually exists. Since Meinesz' startling discovery of one such belt in the East Indian Archipelago (Meinesz, Umbgrove, and Kuenen, 1934), similar belts have been determined by Matuyama (1936) along the west side of the deep Japanese trough and by Ewing and Hess (Hess, 1938) along the unstable front of the Antillean arc. Kuenen (1936) has demonstrated in dimensionally adequate experiments that this process is mechanically possible.

We may assume, then, that the crumpling of the outermost skin of the earth along an orogenic zone is the superficial result of a doubling-under of the crust along a zone of weakness. So long as compression is in full force, the crumpling belt shows little upward tendency. Horizontal movement prevails. As the compressive stresses wane, the thickened downward bulge of light rock materials rises into a position of isostatic equi-

[3] A number of sheets giving detailed soundings and contours based upon them will be issued by The Geological Society of America in Number 7 of the Special Papers series.

librium. This corresponds closely to the actual record. But how about the broad geosynclinal belt which by its pressure localized the orogenic deformation? The deep narrow furrows that regularly lie on one side of a rapidly rising welt may well be part of the dynamic pattern of the orogenic deformation. But the broad geosynclinal belt, in which thousands of feet of limestone, radiolarian cherts, and fine-grained clastics accumulated conformably, can hardly be of the same origin. They seem to belong to another order of things.

OCEAN BASINS AND CONTINENTS

GENERAL STATEMENT

The last of the basic patterns is that which divides the earth's surface into oceanic and continental areas. Two fundamental facts must be accounted for.

"CONCORDANT" AND "DISCORDANT" OCEAN BASINS

The first is the conspicuous contrast that exists between the coasts of the Pacific Ocean and those of the Atlantic and Indian oceans and their Antarctic connection. The outline of the Pacific is throughout its length parallel to the main structural lines which everywhere are those of young orogenic belts. As a unit, the Pacific Ocean may be called *concordant*.

The other oceans cut conspicuously across the major structure lines of the adjoining continents. Opposite shores bear the sort of relation which would be observed if the lands north and south of the Caribbean Sea were submerged so that an ocean 5000 miles wide and 3 miles deep covered parts of the two Americas with opposite shores running NE-SW through Lake Superior in the north and the Central Amazon River in the south. The pre-Cambrian of Laurentia would match that of the Brazilian coign; the plate of Paleozoic sediments of Wisconsin that of the Amazon; the thick Cretaceous-Tertiary sediments east of the Rockies those east of the Andes, and so forth. This sort of parallelism exists between South Africa and South America. The Atlantic and Indian oceans and the corresponding part of the Antarctic are *discordant* [4] seas. And, at least in the case of the Atlantic, there is a truly remarkable parallelism of opposite shores (Pickering, 1907, p. 29, fig. 4) which extends to its northern branches—the North Sea in the northeast and the waters from Davis Strait to the Beaufort Sea in the northwest, including the shores of the islands in the Arctic archipelago.

It looks as if large areas of the earth's surface had torn apart and

[4] Suess (1888, p. 256), who first recognized the importance of this difference, used the terms "Atlantic" and "Pacific" type of coast line.

separated. Wegener (1912; 1915) had the courage to say it does not only look that way, but this is precisely what happened. A single parent landmass tore, and its fragments drifted apart. He had to postulate that the rocks of the ocean floors were materially different from those of the continents in their behavior toward long-applied stresses. But the experience of the last quarter century makes that postulate untenable. The exploration of the sea floor by means of echo sounding has revealed large areas where as much vertical relief exists as on land. Even more decisive is another experience which introduces the second fundamental fact concerning ocean basins and continents.

THIN SIAL BENEATH "DISCORDANT" OCEAN BASINS

The analysis of earthquake records which is now systematically carried on by many competent seismologists shows beyond reasonable doubt that the floor of the *discordant oceans* consists of the same relatively light rock materials that make up the continents, in contrast to the Pacific which seems to be practically devoid of them, at least in its main portion. The thickness of the so-called "granitic" layer is, however, somewhat less beneath the oceans than beneath the continents (Gutenberg, 1934). This is to be expected in a crust dominated by the tendency to maintain isostatic equilibrium. But there is abundant evidence that at least parts of the Atlantic and the other discordant seas were land as late as late Paleozoic time. Ewing's pioneer study off the Atlantic coast shows the peneplained surface of the Piedmont descending evenly to oceanic depths (Ewing, Crary, and Rutherford, 1937).

The discordant character, the parallel shores, the thinner "granitic" crust, and the successive submergence of more and more of the surface of the Atlantic would find a simple explanation if we could assume that the Atlantic Ocean marked a portion of the earth's crust that had been stretched. This has been suggested as a possible solution by so competent a geophysicist as Gutenberg (1936). No better solution has been offered, but it cannot be a continuous process, as he thinks. That would leave no explanation for the great Appalachian belt of folding, especially its east-west continuation in the Ouachita Mountains. It would leave unintelligible the active deformation in the Antillean Arc which the Princeton-Navy Expeditions under Meinesz, Ewing, and Hess have brought to light (Hess, 1938), and also such features within the Atlantic proper as the strong fore-deep which the *Meteor* has found in lat. 20° S. and long. 25° W., lying alongside a strong submarine range that crosses the Brazilian basin diagonally (Pratje, 1928). All these features point to strong compression within the region of the Atlantic.

A PULSATION HYPOTHESIS

Three times we have met the same contrast: Emergence and rejuvenation of continental surfaces in place of an earlier submergence and peneplanation; mountain making on the site of former geosynclinal sinking; evidences of compression on oceanic surfaces that bear earmarks of tensional origin. These contrasts become intelligible if we assume that the intensity of compression in the crust does not merely change from greater to less and vice versa (as most geologists are willing to admit), but that at intervals it actually changes sign and becomes negative, turning from crustal compression into crustal tension[5] (Bucher, 1924; 1933). This would mean that the deformation of the crust proceeds in a great geodynamic pulse:

(1) In the diastolic phase the subcrustal volume of the earth expands; the thin film of the earth's crust pulls apart along belts where through the interplay of many factors it happens to be weakest. Belts that lie near sources of abundant sediment cause the underside of the "granitic" layer to be depressed abnormally in a single tensional phase. We call them geosynclines. Belts of stretching that do not receive abundant sediments have their surface lowered without any bending down of the underside of the "granitic" layer.

In a systolic phase the subcrustal volume of the earth shrinks, and the crust is placed under compression. It buckles downward where the load of sediments has given an initial bend to the under side of the granitic layer and thereby predestined buckling along the geosynclines. As the shortening crust bends downward, the inert outermost film is sheared off, thrown into folds, and piled into thrust sheets. While compression is on, horizontal movement dominates; isostatic adjustment is prevented more or less. But as soon as compression wanes, adjustment is quickly established, causing the characteristic vertical movements. It is possible that convection currents arise below the crust, especially where the contrast is large between the ocean basins with their thinner sialic crust and the continent (Meinesz, 1934; Meinesz, Umbgrove, and Kuenen, 1934, p. 195).

The last of the major elements of the structure of the earth fits simply into the picture if we add one more consideration. While during a compressional phase a large portion of the stresses is taken up by the weak geosynclinal belts, each one of the vast areas of the crust that lie between them is moving centripetally downward with each particle crowding against its neighbors. The result must be a pattern that represents

[5] Staub (1928) recognized the necessity for assuming a reversal of sign, but he ascribed the movements to alternating "Polflucht" and "Poldrift".

a compromise between gravity and friction. The basin-swell pattern would fit such a concept.

Moreover, given such a mosaic of basins with irregular residual swells between them, it is to be expected that during times of compression there would be a summation of residual stresses in the large areas between the great orogenic belts in such a way that roughly equal distances apart the lines of swells would suffer more severe compression than the others. One might call these lines "primary crustal folds" in contrast with those that arose secondarily from geosynclines. They should, by definition, bear a definite relation to the broad crustal pattern. The most conspicuous is the Middle Atlantic ridge which runs faithfully parallel to the great Cordilleran orogenic belts, running along the middle of the Atlantic through its whole length. Its continuation in the Antarctic sea, from which the Kerguelen Islands rise, maintains similarly a position near the center between Africa and the Antarctic continent. Hans Cloos (1937) has pointed out that the great bowed-up highland belt of east Africa occupies a similar position. The same might be said of the highland belt of eastern Brazil and perhaps even of the late Tertiary rejuvenation of the Appalachians.

(2) The sum total of many diastolic phases is successively migrated geosynclines and widened ocean basins; the sum total of many systolic phases is a thickened "granitic" crust, successively folded belts that stand high because of the thickening of the light crust beneath them.

(3) During a compressional phase when the crust is "tucked under", so to speak, the relative position of the continental masses with reference to the earth's coordinates is changed; during a tensional phase, when the landmasses are pulled farther apart, their relative positions change again. And each time they change precisely in the direction visualized by men like Taylor (1910; 1930)—toward the great zones of folding and away from the areas of the "discordant" oceans.

All the while, the larger units of the earth's crust adjust themselves to the changing surface and the shifting masses, moving past each other differentially along large fracture zones like segments of the Antarctic shelf ice—along fracture zones such as Hess infers along the sides of the Bartlett trough or such as the incomparable San Andreas rift which Byerly believes can be shown to be nearly vertical at a depth of 30 kilometers.

(4) The present condition of the earth's crust is one of compression in a minor systolic phase near the end of a major period of dominant compression (Bucher, 1933, p. 482; Stille, 1935). In contrast with the Early Tertiary, when they were intense and worldwide, horizontal movements are now relatively weak and limited largely to parts of the great

orogenic zones, chiefly those of arcuate pattern. Spasms of isostatic adjustment have followed one another with increasing rapidity since the end of Miocene time, as shown by the successive rejuvenations; that is, at progressively shorter intervals, intensity of the compressive stresses has dropped temporarily to near zero so as to permit far-reaching isostatic adjustments.

The fact that at the present the condition of stress is essentially the same in all parts of the earth is shown convincingly also by the definite geometrical relation that exists between the loci of deep-focus earthquakes and the deep-sea furrows on the margins of the Pacific Ocean (Gutenberg and Richter, 1937) which, if Meinesz' interpretation is correct, indicate compression by their very existence (Meinesz, Umbgrove, and Kuenen, 1934, p. 119).

CONCLUSION

There is a long cry from the skeleton outline of such a broad hypothetical concept to its application to the interpretation of the intricacies of local structure which are so dependent on secondary consequences of the major processes. Ultimately the usefulness of any hypothesis will be judged by its ability to take up consistently into its framework all the subordinate tectonic processes. But even if this hypothesis proves useful, the question will be asked: What kind of forces could cause such a pulsation in the earth's crust? I am obliged to answer: I do not know. But I am not much concerned, because I am convinced that such a force will be found if the facts demand that it exists. Fifty years ago the physicist denied the possibility of a length of geological time such as the facts demanded. Let us scrutinize our basic facts of observation and the logic of our reasoning. The causes will take care of themselves.

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THE ROLE OF MINERALS IN THE PRESENT INTERNATIONAL SITUATION

BY C. K. LEITH

(Anniversary Day Address)

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THE PROBLEM

From the beginning of history, nations have differed in their physical environments and therefore in their economic opportunities. Their activities have been influenced and limited by climate, soil, topography, and accessibility. Only in the last century have mineral resources loomed as an important element in man's environment because they were not used on a large scale until the Industrial Revolution. The world's mineral supplies are large enough for all if they were equally distributed among the nations, but unfortunately they are not. Nations favored by their possession of mineral supplies necessary for industrialization are the ones which in modern times have taken a commanding position in world affairs. Military power is coming more and more to be measured in terms of guns, ships, automobiles, and airplanes, rather than by man power alone, and these implements are in turn limited by the capacity of the industrial plants and the raw materials to feed them.

The control of the larger part of the world's mineral wealth now centers in nations bordering the North Atlantic Basin, where the Industrial Revolution started. The possession of coal and iron and accessory minerals permitted the huge industrial development of these countries, and from this development started the principal exploitation of the supplementary mineral resources in other parts of the world. A great preponderance of the world's industrial power is concentrated along an axis extending from the Great Lakes in the United States eastward through Central England

and West Central Europe, and most of the mineral resources of the world are tributary to this power axis. England and the United States alone control in about equal proportions nearly three-fourths of the world production of minerals; not less important, they control the seas over which the products must pass. Classed with the "have" nations also are France and her colonies, and Russia.

Discovery will doubtless here and there modify the picture, but the rate of discovery is falling rapidly, and geologists, I think, are pretty well agreed that the salient features of the world's mineral distribution are fairly well known.

The position of the nations dominant in the mineral trade is now being seriously challenged by the less privileged nations, including not only the dictator nations, but others like Mexico, Chile, and Brazil, where nationalism is rampant. Three nations have announced by word and deed that there can be no permanent peace until natural resources have been redistributed among the nations. This demand is sometimes camouflaged by political objectives based on historical and racial considerations and the general desire for national prestige. In the last analysis, however, the acquirement of larger supplies of raw materials is a basic prerequisite to the accomplishment of these ends, and the dictators at least are frank to say so. Minerals are not the only raw materials sought, but they are the spearpoint of the demand.

The challenge is not temporary and not merely due to the whim of individuals but is based on conditions which will mean greater intensity in the drive during a long future. When population pressure outstrips capacity for food production, the only way to maintain or improve the standard of living is to provide means of purchasing food outside through the growth of industry or through the forcible acquisition of more territory. The military power necessary for national expansion is also based on industrial capacity. With the rapidly increasing scale of industry the demand for the necessary raw materials becomes concentrated on the few large sources of mineral supplies capable of meeting the new scale. These sources are far too few to go around among the nations; hence, the growing dependence of the deficient nations on more distant sources of supply, and the growing necessity of controlled access to them in peace and war. Partial success in obtaining supplies merely builds up the demand for more, and there is no guarantee that the process would stop even with complete equalization. If all the mineral supplies of the world outside of the British Empire and the United States were to be included, they would be less than those of the dominant group. Opportunity to purchase the necessary supplies during peace times is not regarded by the "have-not" nations as sufficient protection to their national interests, partly because

the terms are dictated by the more fortunate nations, partly because the means of purchase are limited, but very largely because the supplies could be shut off during war or during economic boycotts.

It is not easy for us living in a nation abundantly supplied with mineral resources to realize the urgency of the demand by the less privileged nations, and we are likely to regard their efforts as marauding which must be suppressed. We have been more or less smug about our favored position and have failed to make even serious efforts to solve the problem of equalizing access of the less favored nations to natural resources. If we were put in the same position as the "have-not" nations, however, the cause doubtless would seem a more reasonable one based on real needs, to be supported by force if necessary.

The challenge of the "have-not" nations has taken various forms. First is the effort to find and develop domestic supplies and to find processes which will make domestic low-grade supplies available. All possible substitutes are used for minerals in deficient supply. Such efforts have gone far enough to demonstrate their essential futility. The inevitable consequence is the pressure for control of more supplies outside through acquisition of territory. Parallel with the drive for more territory is the drive for commercial control of minerals, principally by the method of barter, and also by blocked exchange and bilateral treaties. The barter campaign is proving successful in various quarters. It will not solve the problem of insuring self-sufficiency in time of war but it will help in this direction.

The nations rich in mineral resources are therefore confronted by a threat which cannot be met by a policy of laissez faire without seriously jeopardizing their positions in the world. Affirmative decisions will have to be reached which will profoundly affect the question of future standards of living and of peace and war. I do not presume to know the answer but will present certain alternatives which seem to be inherent in the situation.

Before discussing these alternatives, however, I shall review briefly some of the recent efforts of Germany, Italy, and Japan to improve their mineral positions.

ILLUSTRATIVE CASES

GERMANY

Before the war, when Lorraine was still a part of Germany, 92 per cent of the German requirements for iron ore were obtained from domestic sources. With the loss of Lorraine, Germany began an intensive development of her miscellaneous low-grade deposits and of scrap, which together now contribute about a third of her total requirements for iron. She plans to bring this proportion up to about 50 per cent in 1940 by the use of ores at Salzgetter and Linz (Hermann Goering Company), ores of

such extremely low grade that the operation must be heavily subsidized. Already the cost of producing pig iron from domestic ores is double that from higher-grade imported ores. The iron ore gained in Austria bettered the German iron ore position by only a small percentage. Czechoslovakia is itself an importer of iron ore, and therefore its domination by Germany merely adds to the German deficit. Northern Sweden has become the principal source of German imports, and so vital has become Germany's dependence on these ores that their control looms as a major strategic problem in case of war. The alternative source within potential reach is the Ukraine.

Germany is restricting imports of oil by the development of domestic oil resources and the manufacture of oil products from coal. The final production cost of gasoline is now roughly four times the world market price. In 1938 she met about 38 per cent of requirements from domestic sources, and huge expenditures are being made in the hope of bringing domestic production up to about half the total requirements in 1940. The nearest source within striking distance is Rumania.

The German production of copper, mainly from the Mansfeld copper district, amounts to about 14 per cent of the need. She hopes to raise this output up nearly to 25 per cent by 1940. The industry is very heavily subsidized, and the cost of copper is far above the world price. At best it seems unlikely that Germany can ever be free of dependence on foreign sources for nearly three-quarters of her supply. In her effort to avoid the importation of copper, Germany has decreed the substitution of aluminum, magnesium, and other light metal alloys, wherever possible, but the total effect of such substitution is only a minor reduction in the needed quantity of copper.

Germany is also dependent on foreign sources for part or all of her normal requirements of aluminum, antimony, chromite, lead, magnesite, manganese ore, mica, nickel, phosphates, quicksilver, sulphur, tin, and tungsten.

I may summarize by saying that the net result of the autarchic effort leaves Germany still dependent largely, in some cases wholly, on foreign sources for its requirements of at least 15 industrial minerals. The deficiencies in iron, oil, and copper are the most critical because of the volume required. Most of the remainder are used in small enough volume to permit the accumulation of stocks sufficient to tide over short emergency periods, though perhaps not enough to carry the nation through a long war.

Offsetting the gains Germany has made are the growing costs and difficulties of securing the minerals still needed from foreign sources, owing to the growing handicap of exchange. The effort in minerals is only a part

of the general movement toward economic nationalism which is limiting the exchange of goods and Germany's capacity to pay for foreign raw materials. Because of exchange, also, Germany's purchases are being transferred to secondary sources of mineral supply.

If the German domestic effort toward self-sufficiency in minerals promised real success, there would be less pressure for the return of colonies or for the acquisition of political control of other territory, or for expensive barter. It is precisely because it is not succeeding and is at the same time narrowing access to the world raw material markets that Germany is intensifying its efforts to secure raw materials, by one means or another, outside of its own boundaries.

ITALY

Italy's domestic production of iron ore amounts to only about 25 per cent of her total consumption of iron and steel. The percentage is being raised to about 35 per cent by a greater use of pyrite ash, by the utilization of low-grade iron carbonate deposits, through the roasting process, and by the treatment of ancient iron slag deposits. Nevertheless, Italy cannot eliminate her essential dependence on foreign iron sources.

The coal produced in Italy is only about 8 per cent of the country's total requirements, and all coking coal for the steel industry must be imported. To reduce imports, a greater use is being made of domestic coal, with its high sulphur and ash content; and electric power is being substituted wherever possible. But any increase in the domestic output of coal will be at a high cost and at best will reduce imports only slightly.

Domestic crude oil production, including some from Albania, amounted to only 4 per cent of the country's requirements in 1938. Hydrogenation plants, one of which will use lignite as its principal raw material, are being built. Here again the result will be only a minor increase in domestic production, achieved at high cost and without materially reducing the need for imports.

As for copper, Italy produces less than 4 per cent of her total needs. All possible substitutes are now being used; therefore, dependence on foreign copper sources will continue. Her present need for foreign manganese will, she hopes, be reduced materially by the development of her own very low-grade manganese ores, though here again costs will probably be excessive. One-third of her lead comes from foreign sources.

Italy has not been able, then, materially to reduce her dependence on foreign sources for most of her iron ore, coal, copper, and crude oil, and for all her chromite, manganese, nickel, tin, and tungsten. The critical minerals—critical because of the large quantities in which they are required—are coal, copper, iron, and oil. And, it is worth noting, each of

these is needed in time of war in much larger volume than during peace. The conquest of Ethiopia has brought Italy no mineral resources of importance, nor are the prospects bright that there will be further discoveries in that quarter. The iron ores and other minerals of French and Spanish North Africa are obvious objectives of any expansion in the Mediterranean.

As in the case of Germany, Italy's campaign for autarchy and the resultant scarcity of foreign exchange are making it increasingly difficult for her to purchase the supplies she needs from abroad, except by barter.

JAPAN

Japan mines within her own territory only 35 per cent of the iron ore needed. The conquest of Manchuria placed the political control of extensive iron deposits in Japan's hands, but the ores are of very low grade, requiring concentration, and yielding a product too high in silica. The occupation of North China added more low-grade ores of the same type and the sedimentary ores of the Hsuan-Lung in Chihli province, where there are from 25 to 50 million tons of fairly good grade hematite ore, only partly proved, and some of it inaccessible to cheap transportation. On the Yangtse River in Central China, Japan has acquired control of a group of contact magnetite deposits aggregating perhaps 75 million tons of good grade which is the only group of iron ores in China mined on a modern scale. These gains have temporarily improved Japan's iron ore position, but the total reserve is far short of that necessary to furnish an adequate supply of iron ore for the future. At best it would supply the United States for scarcely 2 years. Farther afield, Japan has secured control of small ore reserves in the Malay States, which are now the principal source of import to Japan. She secures a small tonnage from the island of Luzon in the Philippines and has attempted to secure control of the large laterite deposits in the island of Mandanao. The Philippine government, however, has refused to lease these deposits and is now developing them for sale of ore to Japan. Japan started the exploitation of the iron ores on Yampi Sound in northeastern Australia, but this operation was taken over by British and Australian interests, and in the summer of 1938 export of iron ore was prohibited by the Australian government. Similar action was taken by the French government in Indo-China. As the situation stands, the only solution to Japan's iron ore problem would seem to be access either to the Philippines or Eastern India.

Japan's domestic coal supply is nearly adequate to her needs when measured in total figures, but it is inadequate in terms of coking coal and anthracite. These deficiencies are supplied by the acquirement of Manchuria and North China coals.

Japan produces only about 7 per cent of her petroleum needs. At

present she is establishing plants in Manchuria for the extraction of oil from shale. She also has a concession in the Soviet part of Sakhalin from which she is obtaining petroleum and another in Dutch Borneo which is not yet in production. She is also building a plant for the recovery of oil by the hydrogenation of coal. Oil production, refining, and importation are all subject to government license, and the important oil companies are required to keep in stock an amount equal to half their imports of each item during each year, such stocks to be sold in case of emergency to the Japanese government at its own price. The net result of these activities still leaves Japan dependent for the major part of her oil requirements on imports, principally from the Netherlands East Indies and the United States. Probably the major strategic problem of Eastern Asia is the control of the oil of the Netherlands East Indies.

Japan has the largest reserves of copper ore in the Orient; she nevertheless has to import a considerable quantity of that mineral.

In occupying territory back of Hong Kong, Japan has acquired control of the antimony district of China which has supplied half to three-fourths of the world consumption. Also, it has cut off the flow of tungsten through Hong Kong from South China and is in striking distance of the tungsten-producing territory which has supplied a large part of the world requirements.

Japan's critical shortages, then, are in iron ore and oil. In addition, she is largely dependent on imports for her aluminum, chromite, lead, nickel, manganese, phosphates, potash, tin, tungsten, and zinc supplies, and to a minor extent for copper. As in the case of Germany and Italy, exchange difficulties are limiting access to foreign supplies, thereby reenforcing the expansionists' demand for the extension of Japanese political control over foreign sources.

POSSIBLE SOLUTIONS

From such facts as these, and there are more, it seems a fairly safe conclusion that the results of the autarchic efforts in the field of mineral resources are falling far short of their objectives. The inequalities of mineral distribution among the nations are stubborn facts which cannot be greatly changed by wishful thinking or political measures. On the other hand, there is no indication of any cessation of the effort. The problem will not be settled by any changes pending in present disturbances in Europe or Asia. It is larger than that and will grow steadily more acute until the ultimate question of world control of raw materials is settled.

How shall the problem be met? By a policy of appeasement, through division of our political and commercial control of resources, or by defense of the status quo?

Whatever theoretical merit there may be in the policy of appeasement, an examination of the actual possibilities seems to make this course impossible from the standpoint of practical politics. The cession of all the colonies would not accomplish much for the reason that the sum total of their mineral resources, which have been carefully listed, is far short of the requirements of the "have-not" nations. The acquirement of Abyssinia, Austria, Manchuria, and large parts of China and Czechoslovakia have done little to meet the mineral requirements of the "have-not" nations. Real appeasement by this method would require the ceding of considerable parts of self-governing nations, including English-speaking, French, and Russian domains, as well as supplies controlled commercially by the English-speaking people outside their borders. Still further, it would mean division of control of the sea to assure continuity of movements from distant territories. In short, the scale of the transfer is unthinkable from a political standpoint, and it seems to be a safe assumption that before it goes much further it will be resisted by the armed force of the "have" nations, as illustrated by our recent announcement of policy to protect the Western Hemisphere.

Much is said about the possibility of appeasement through elimination of trade barriers and restoration of free circulation of raw materials between countries. The effort being made by means of the Hull reciprocity treaties is regarded in many quarters as a possible solution of the problem. It certainly will help. In essence, however, it is merely an effort to restore the old status in which the balance of power is retained by the "have" nations by virtue of their possession of the larger share of the world's mineral resources, and it leaves the "have-not" nations in their old inferior position. This is just the control that is now being challenged. In a recent speech, Dr. Hjalmar Schacht, President of the Reichsbank, called this procedure "the recipe of the well-to-do", who in "the fullness of their riches cannot understand that a poor nation still has the courage to live according to its own laws." Appeasement by this method, to be effective, would have to go further than anything yet seriously planned. For instance, our own country has been committed to a tariff designed to preserve our standard of living, which makes it difficult for the "have-not" nations to sell us enough of their products in return for the needed amount of our raw materials, and it seems most unlikely that it will be politically possible to eliminate enough of these barriers to solve the problem of the easy flow of raw materials.

Ultimately appeasement may have to include some sort of collective guaranty of equality of access to raw materials. This would require the public recognition by the "have" nations of the concept that their fortunate raw material position is one of trust to be administered not only for their

own material advancement but for that of others, thereby lessening causes of world friction. The practical working out of this concept is enormously difficult. It would require a degree of objective thinking and of correlation of activities at home and abroad which is far beyond anything yet attempted or regarded as practical. Considerations of temporary and local self-interest will certainly postpone the attainment of this Utopian goal too long to be of material help in solving the immediate problem. The fact that the defense of our material position is so bound up with our defense of democratic institutions in itself is likely to postpone constructive effort in favor of armed defense of the status quo.

My conclusion, reached with reluctance, is that appeasement for the present will fail, that the privileged position of the "have" nations will be defended, that ultimate control will remain where it is, that might will continue to make right. The necessity of protecting democratic institutions will greatly stiffen the defense of our material position. The military preparation for this defense is already well under way. The preparation of defense against economic penetration is still sadly in arrears but will doubtless be formulated when and as forced by the course of events. The application of mineral embargoes to disturbing nations is a not unlikely outcome. It seems probable for political reasons that the necessary degree of cooperation between the "have" nations for mutual defense will not be reached until further inroads have been made on their mineral domain; in other words, that cooperation will be defensive and not the result of an affirmative effort to organize in advance in the hope of limiting future conflict and maintaining a certain amount of law and order by force. The ultimate problem of finding some way to administer the world's mineral resources from the standpoint of world welfare and peace remains for the future.

POSITION OF THE UNITED STATES

The United States is the world's largest producer, the largest consumer, and the largest distributor of minerals and their products. The frontiers of our mineral industries, including sources and markets, extend to nearly all parts of the world. It is part of *our* domain that is ultimately at stake in any struggle for the redistribution of mineral resources. Even the hazard involved in any attempt to maintain neutral shipping rights in this field may be enough to involve us in the struggle.

The self-sufficiency of the United States in raw materials is relative, not absolute. If all our imports were cut off, our industry would indeed return to the "horse and buggy" days. We could build neither an automobile nor a battleship. Our deficiencies are mainly in the so-called ferroalloy group of minerals used in the steel industry, including chromite,

manganese, nickel, tin, and tungsten. There is also a lack of antimony, mercury, certain varieties of mica and graphite, as well as of other minerals. The ramifications of use of all these minerals are so complex in modern industry that the lack of a single one often has far-reaching consequences. It is not merely a question of relinquishing a small percentage of our world trade or of taking an egg from a basket in which there are many more of the same kind. It is more like taking a wheel from a watch or an automobile. Our standard of living is definitely affected, and we are likely to resist the change, even though recognizing the appeal of a policy of isolation as an alternative to war.

The United States shares with other democratic nations both the immediate problem of defense of its material and ideologic position and the longer-range problem of alleviating the raw material grievances of the "have-not" nations in the interest of world welfare and peace.

Geologists cannot settle these questions, so vital to world peace, but, knowing as they do their physical background, they are in a position to make highly significant contributions to both the immediate problem and the long-range problem of using our mineral power in trust for world welfare. The responsibility should not be avoided.

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CONTRIBUTION OF GEOLOGY TO SHAPING OF IDEAS ON THE MEANING OF HISTORY

BY JOHN C. MERRIAM

(Anniversary Day Address)

However opinions may vary regarding the meaning of history, with relation to specific situations there seems to be general agreement concerning its significance, in the characterization of an important dictionary, as "devoted to the exposition of the natural unfolding and interdependence of events." Ideas concerning interdependence of events in series are, of course, only one means of indicating that we find it difficult to conceive of successions of things and events without expression of interdependence which suggests cause and effect.

Present-day thought furnishes varied opinions as to the extent to which human history may be used as the basis of prediction. It shows much less difference of opinion regarding development of existing situations out of the past, or concerning the desirability of such formulations of knowledge as would assist in attempts to plan the future intelligently.

The realization by all thinking persons that present world conditions involve an enormous volume of complicated factors touching various kind of interests, many lands, and long periods brings home to us the need for greatly increased and more accurate information, as also of clear relation of these factors, to one another. As the range and difficulty of problems increase attempt is made to meet the situation by more intensive research, better organization of knowledge, and better judgments, involving both the masses of facts and the broad principles essential to synthesis and interpretation of knowledge. Among the questions raised often is that concerning precise values to be placed on historical succession of materials. Other problems arise in the evaluation of individual situations with relation to the broad and deep background of forces and influences, which seem so important in the scheme of the world as we find it.

Today we see this problem at times in terms of what is called planning or foresight, or such organization of information and program as will make use of all available values. Commonly we wish to look as far ahead as available knowledge and understanding permit.

The present period in human affairs is characterized by necessity for

exceptional breadth of vision and knowledge in practically every field of interest and activity. Whether the problems concern business and economics or social and governmental situations, or whether they relate to questions touching science, education, art, or religion, invariably the drift of inquiry turns to consideration of things involving relationships of world-wide extent. The rapid increase in magnitude and complication of world affairs in recent decades has unavoidably become a matter of concern to everyone, and there is naturally extensive search for causes and remedies.

In meeting the needs of present situations requiring wide vision and recognition of varying conditions, we have come to full appreciation of the necessity for acquaintance with the values inherent in geographic or space location. We understand the importance of reckoning with these factors in any effort aimed at attaining acquaintance with the world as it really exists.

Appreciation of simultaneous existence of differing regions and climates, peoples, and governments is made easy by the near-miracles of communication and transportation. As a consequence the world is learning to visualize the differences among situations and problems regarding which the citizen must, from time to time, express opinion, perhaps in casting a ballot. In spite of our apparent tendency as a nation to emphasize the assumed security of our separateness, there is increasing appreciation of the fact that it is not possible to avoid the implications of relation to other parts of the world and the consequences of such a situation.

It is, however, with greater difficulty that we come to visualize succession in time of situations and conditions, the relations of which we have learned to appreciate when expressed in space, though we are no less certain of the reality of such differences in the connected time series. We have even begun to understand that the time series commonly represents trains of things or of circumstances which are not only connected, but express relationships in the sense of natural unfolding described as growth or development. To a certain degree we find it desirable to examine these time series of things or situations, or events, with a view to learning in how far they may express definable laws of cause and effect, or of evolution, conceivably of value to us in attempting to understand the present or shape the course of the future.

Considering these questions from the point of view of science alone, it is interesting to note that as thought advances there is increasing dependence upon the idea of unity in nature, comprising both time and space, until it has come to be seen not only as an increasingly practical element in research programs, but as one that must be considered in

research on many of our most important problems. It is no longer possible to study the broader questions of astronomy without recognition of interrelation among the elements of the universe about us. Again, the solution of many problems of biology depends upon recognition of interrelation among elements in space and time in a manner emphasizing connection between the factors represented, such as appears so strikingly in what we know as evolutionary series.

Even when our minds are concerned with situations representing human affairs only, we realize that a great number of factors are involved in which the successions or sequences seem to have relation to expression of great forces in human relations and in nature, so that a vast complex develops which is often so difficult to visualize that there seems little hope of understanding it fully. It is nevertheless interesting to note that as we define and lay out the factors recognized in both space and time, broadly comprehensive principles appear that develop the meaning of these features, both large and small. We realize now that only rarely may human situations be found that can be considered satisfactorily as separate from connected antecedent conditions or from coincident situations of other regions perhaps related by connecting series of preceding stages.

One purpose in view in this paper concerns the idea that history has meaning and illustrates truths of great importance to us, as contrasted with the idea that it has little significance or perhaps none at all as guide for planning or conduct. Investigation will reveal many meanings depending upon the views taken or the questions under consideration. This subject is considered with the idea that study of the problems of history may be of increasing value as attempt is made to solve vastly difficult and complex questions involving wider and wider relationships and longer stretches of both the past and the future.

As one looks out over the fields of knowledge which human intelligence has assembled, among the materials expressing flow of time the wider story of earth history appears as the outstanding illustration. Covering as it does the longest accurately known, or in a sense experienced, span of recorded time, it may be expected to bring into interpretable relation to one another a broad range of elements in nature in such form as to make possible wide-reaching and fundamental conclusions regarding laws representing habits or modes of procedure in nature. At the same time one might expect this story to furnish much evidence concerning relation of the living world to the physical environment in which it has developed. It might also be assumed that in the complex of materials presented there would be abundant information regarding the story of human kind, both as concerns its relation to nature and with reference to steps in the devel-

opment of man as it may be related to varying conditions in space and time.

The vast panorama presented in the geological picture can be broken down into a great number of close-up views as seen by different branches of science. There is advantage in study of these separated and limited representations, but there are also large values in the picture as a whole, presenting materials and events with their widest scope, longest range, and clearest perspective.

Not the least important of the values coming from a study of the geological story is the picture presented by successions of events in which there is apparent the continuing effect of great forces which we understand only in part, but which are evidently enormously important in the development of the earth and of its inhabitants. Without knowing precisely what these forces or influences are, we may be aware of their existence and of their effects. In following the geological story, we may be deeply impressed by the fact that these things of such tremendous significance are involved in, or stand behind, the movement of nature, bringing about or guiding development of features which were ultimately moulded into forms which we find important or outstanding in the world of the present.

The influence exerted by examination of this sequence of factors invariably has its effect in making clear to us that we exist in a world which has developed out of a long past, through ages in which the dominating elements were forces beyond our understanding or our full appreciation. It is not to be assumed that recognition of these factors will have a disturbing influence upon human thought. Rather, it may indicate that we live in a world much of which is beyond our understanding as to its nature and its operation, though we see the working out of these forces in what we consider an orderly manner. In other words, we recognize the present as a development out of a long and complicated period through orderly movement of events determined by great forces sometimes appearing to be inimical to what may be termed the human interest, but which may be looked upon as important to us by reason of their orderliness, and because they have produced situations which we have considered of great significance for us.

Visualizing history as placing before us the significance of those developments which appear in the sequences of change in time, the broader story of geology seems to offer exceptional opportunity for study of what movement and sequences bring about with the passage of time. If we are to attain that devoutly to be wished for stage in development of learning and its practice at which study of movement in world affairs may in some measure be aided by use of history, it seems important that among other

steps every effort be made to secure as nearly as possible a complete picture of what geology in all its phases can furnish, and that the elements secured be so represented as to give a true view of relationships between these features in terms of space, power, movement, development, and time.

What has been suggested as a goal of the geological sciences in the general synthesizing of knowledge for human use means in one sense nothing more than what is the everyday duty and experience of the geologist, together with those in associated fields of research. There is, however, another extremely important aspect of this problem, which is that somewhere there must be the interest, talent and the opportunity for presenting the results of these studies in such a manner that they become generally intelligible to interested and thinking people. There must also be opportunity for education, preferably self education, of the people on these subjects through contact with realities which can express their story in a form intelligible, attractive, and humanly interesting.

It is, moreover, important that among those who come into the range of influence of these features of nature presenting exceptional illustration of the story of the earth, there be included as many as possible of the outstanding individuals who ultimately have influence in guidance of the great affairs of the nation and of the world. It is desirable that those who deal with the greater problems of the present appreciate the context of this story as it is expressed by the vast forces and influences of the world of people and things which collectively constitute the materials of the past out of which we have come, and of which in a measure we are a part.

WASHINGTON, D. C.
DECEMBER, 1938.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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ICE AGES IN THE GEOLOGICAL COLUMN

BY A. P. COLEMAN

(Anniversary Day Address)

Our earth is a fairly comfortable little world in spite of the fact that its warmth comes from a furnace 92,000,000 miles away. It is true that its two polar ends are icy, but if the winter is too cold in New York we can go south to Miami or Panama or Colombia and find as warm a climate as we want.

We are apt to think that our present conditions are normal, but that is far from being the case. Our temperatures are far below the usual conditions shown by fossils in the world's past history. More than a quarter of a century ago I climbed a mountain in Spitzbergen, crossing a snowfield and a glacier, to reach beds of sandstone crowded with great fossil leaves of a forest as luxuriant as those of southern New York or Pennsylvania. The tallest willow or birch on Spitzbergen now rises less than a foot with leaves not an inch broad. Similar forests grew in Greenland, and coal and tree trunks occur in Antarctica, now without leafy plants.

A few million years ago the earth was warm to the poles, and paleontologists assure us that the ancient climates of the world have usually been much warmer than at present.

Nevertheless there have been a few times of much greater cold in the world's history—Ice Ages—which have profoundly influenced the life of the world. Four of these have left their records strongly marked in the earth's crust—one in the Pleistocene, from which we seem to be slowly emerging, another at the end of the Paleozoic, a third at the end of the pre-Cambrian or the beginning of the Cambrian, and a fourth low down in the Huronian.

The Pleistocene glaciation is to be taken up by my friend, Dean Kay, and my brief talk will be devoted to the others; first a few words must be said of the work of ice, so that we may recognize its effects in the past.

The ice of a glacier is a hard and brittle solid that can support any load of rock rolled down from the mountain side, yet under the pull of gravity it moves slowly down like a plastic material such as pitch. It piles up the stones it carries in a heap at the end, called a moraine, grinds off and polishes the rock beneath, and leaves behind "rock flour" mixed with "striated stones" forming boulder clay or till. Till consolidated to

rock with its striated stones is the best evidence of ancient glacial action.

I have been studying ice work for the last 40 years and still find the work fascinating. The study of glaciation gives a good excuse for going almost anywhere from the Arctic regions to the equator. It has led me to several points in Mexico and the Andes and to Africa, Australia, India, and South America.

The most astonishing of the ice ages is that of Permo-carboniferous times, at the end of the Paleozoic, when ice sheets covered many thousands of square miles of country now within the tropics.

More than 30 years ago I tramped in very hot sunshine over the Karroo desert in South Africa and collected beautifully striated stones, now in cases in the Royal Ontario Museum, Toronto, and I sweat even more while collecting glaciated stones on Penganga River in the heart of India a quarter of a century ago. One may see wonderful beds of ancient boulder clay near the Newcastle coal region in Australia, and I have been laughed at by country people riding their mules to market while I chipped striated stones from tillite well within the tropics not far from plantations of coffee and bananas in Brazil.

Experiences like those just mentioned give a geologist reasons for furious thought. What sort of climate had the world when ice sheets reached the tropics in three or four continents? Strange to say Boston and Alaska seem to have been the only points ice covered in North America. Conditions seem to have been just the opposite of those in the Pleistocene when North America and Europe were half-covered with ice and the other continents escaped glaciation except on high mountains.

The Permo-carboniferous ice age was much the most terrible known in the world's history.

The ice sheets near the beginning of the Cambrian are known from South Africa, Asia, and Australia and reached farther toward the equator than those of the Pleistocene; we know too little of this very ancient glaciation, however, to dwell longer upon it.

A Huronian ice sheet covered tens of thousands of miles of northern Ontario, and excellent striated stones have been collected in the waste dumps of the Cobalt silver mines. It is chiefly of interest as showing that the earth experienced times of intense cold far back in its pre-Cambrian history when some geologists have thought that it was cooling down from a molten condition.

The earth has undergone several ice ages in the past. What about the future?

At the present time glaciers are everywhere retreating, suggesting that we are slowly emerging from the Pleistocene ice age.

Only about half the area covered by ice has yet been set free; however,

it has been suggested that if the remaining 6,000,000 square miles of ice are melted the level of the ocean will be raised about 150 feet. What effect this will have on the world's seaports, including New York, can readily be predicted.

With a little imagination one may picture Oslo or Rio de Janeiro—seaports with high ground in the rear to which to retreat—as sending palatial holiday cruisers to see New York's deserted sky scrapers rising as steep-walled bird rocks from a shallow sea; these changes go on very slowly, however, from the human point of view, and real estate owners need hardly worry over what may happen in a few thousand years.

ROYAL ONTARIO MUSEUM, TORONTO, CANADA.
DECEMBER 1938.

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PLEISTOCENE HISTORY AND EARLY MAN IN AMERICA

BY GEORGE F. KAY

(Anniversary Day Address)

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USAGE OF THE NAME PLEISTOCENE

One hundred years ago last year, in 1837, Louis Agassiz, then living in Switzerland, advanced the view that there had been continental glaciation in northern Europe. A few years later, in 1846, Forbes, of England, introduced the name Pleistocene to include all glacial deposits younger than the Pliocene and older than the Recent. This usage was adopted by Lyell in 1873 and has continued to the present. Pleistocene history includes, therefore, the time involved in the records of the Great Ice Age.

GRADUAL RECOGNITION OF THE COMPLEXITY OF PLEISTOCENE HISTORY

From the year 1837 until the present, glacial phenomena have been studied on every continent by many geologists, and year by year as investigations have continued more and more of the complex history of the Pleistocene or Glacial Period has been unravelled. At first it was believed that all the phenomena could be explained in relation to the advance and retreat of a single ice sheet. But soon the evidence indicated that there

had been two ice sheets separated by a long interglacial age. This evidence consisted in many places of a forest bed or buried soil separating two tills. Such evidence was found in both Europe and America. Later, and still before the founding of the Geological Society of America, Chamberlin employed differences in topographic form and degree in alteration as bases for differentiating tills. In fact, he put greater emphasis on these features than on forest beds.

By the time of the organization of this Society, N. H. Winchell, McGee, and Chamberlin had recognized three drift sheets in Minnesota, in Iowa, and in Wisconsin, respectively, but no distinct names had been given to any of them. Within the next few years unusual progress was made here in America and in other countries in the interpretation of the complex history of glacial time.

PRESERVATION OF PLEISTOCENE RECORDS IN THE MISSISSIPPI VALLEY

By the year 1900, Chamberlin, Leverett, Calvin, and their associates had discovered that the records both glacial and interglacial of the Glacial Period had been preserved more satisfactorily for study and interpretation in the Mississippi Valley than in any other known area in America or elsewhere (Kay, 1931, p. 427-434). And, moreover, it had become generally conceded at this time that of all the States in the Mississippi Valley Iowa had the most available evidence from which to interpret Pleistocene history. The problems of this limited field would seem to be the main problems of the continent insofar as the Age of Ice is concerned.

GENERALLY ACCEPTED INTERPRETATIONS OF THE RECORDS UP TO THE YEAR 1900

By the beginning of the present century there was general agreement among the geologists who had studied in Iowa and adjacent States that the evidence justified the interpretation that Pleistocene history had been long and complex, embracing five glacial and four interglacial ages, each of which had been described and named. The recognized classification was as follows:

Wisconsin
    Peorian
Iowan
    Sangamon
Illinoian
    Yarmouth
Kansan
    Aftonian

Pre-Kansan

The Iowan drift sheet was interpreted at this time to be related closely in age to the Wisconsin drift sheet, and the widespread loess was called Iowan loess because of its relationship in origin and in age to the Iowan drift. It was at about this time that the loess was being interpreted as eolian rather than fluvioglacial in origin. Soon the loess, on account of the fossils it contains, was interpreted to be not contemporaneous with the Iowan drift but somewhat later in age than the Iowan. With this change in interpretation the loess began to be called Peorian loess. It had been deposited in the short interglacial age between the Iowan and Wisconsin glacial ages.

The published record shows clearly that at this time—about the year 1900—this classification had the approval of all the students of glacial and interglacial deposits of the Mississippi Valley—the classic area.

NEW FACTS FROM MANY AREAS BETWEEN THE YEAR 1900 AND THE PRESENT

The question may now well be asked, What new facts and new interpretations of Pleistocene history have resulted from the additional studies of the records of Glacial Time in the Mississippi Valley, in other parts of America, and in other countries during the last nearly 40 years?

During this time glacial and interglacial materials have been described and mapped on several continents. Laboratory work has supplemented the field work. Profiles of weathering—the gumbotils, mesotils, siltils and other related products of physical disintegration and chemical decomposition—have been thoroughly investigated. Ancient Pleistocene dust-storm materials have been studied, varves have been analyzed and interpreted, and mountain glaciation, drainage changes, the history of the Great Lakes, stagnation and downwastage of ice versus normal recession, and evidences of Pleistocene elevations and depressions are now well known. Attempts have been made to determine the amounts of change of sea level which have resulted from glaciation and deglaciation. The life of Glacial Time has received attention, including the relation of man to glacial and interglacial deposits. Comparisons have been made and correlations attempted between the records of America and of other countries. An extensive literature on the records of the Glacial Period is now available. What, then, have been the results of these many and varied refinements of investigation? What is the present-day consensus regarding the history of this so recent and fascinating chapter of the earth? For an answer let us turn again to the classic area of America, the Mississippi Valley, since here only do we find satisfactory records of the older phases as well as many of the younger phases of Pleistocene history.

PRESENT INTERPRETATION AND CLASSIFICATION OF THE PLEISTOCENE RECORDS

When the most significant facts of known Pleistocene time are analyzed critically in the Mississippi Valley, it is evident that from the beginning of the advance of the first ice sheet over a maturely eroded surface of moderate relief to the present time there have been recurrences of similar geological events with accompanying similar geological results. In other words, there were within the limits of Pleistocene time a succession of cycles, the most distinctive evidences of which are recorded in the deposits which were made during each cycle and in the changes which these deposits underwent before the coming of the succeeding cycle. The records of the glacial and interglacial ages of the different cycles are strikingly similar in many respects but unlike in other respects.

The oldest cycle of Pleistocene history began with the advance of the first ice sheet, since 1909 called the Nebraskan ice sheet, and ended with the oncoming of the second ice sheet, the Kansan. The deposits of the glacial age of this cycle are the Nebraskan till and associated sands and gravels. During the interglacial age of this cycle, the Aftonian age, those parts of the Nebraskan drift which were so situated topographically throughout the age that they were not subjected to erosion underwent profound chemical changes. The most significant and widespread product of this weathering of Nebraskan drift was Nebraskan gumbotil and related materials. Before the coming of the second cycle much of the Nebraskan gumbotil plain was eroded. The Nebraskan drift of Nebraskan glacial age and the Nebraskan gumbotil and related weathered profiles of Aftonian interglacial age are so intimately associated in the field that it is necessary that both of them be studied together in order to determine fully the characteristics, distribution, and history of each of them. In this respect the Nebraskan drift and related weathered materials may properly be considered as constituting a unit of Pleistocene time—the chief sedimentary record of the first cycle.

The second cycle began with the advance of the Kansan ice sheet and terminated with the oncoming of the third ice sheet. During the Kansan glacial age of this cycle the Kansan drift, which in many places is similar to the Nebraskan drift of the first cycle, was deposited; and during the Yarmouth interglacial age of this cycle Kansan gumbotil and related weathered materials were developed widely where the conditions were similar topographically and in other respects to the conditions which produced the Nebraskan gumbotil in the Aftonian interglacial age of the first cycle. This Kansan gumbotil, which was developed by physical and chemical weathering of Kansan till during the Yarmouth interglacial age, is thicker than the Nebraskan gumbotil developed in the Aftonian age

of the first cycle. This and other facts indicate that the Yarmouth age was of greater duration than the Aftonian age. A mature erosional topography was developed on the Kansan after the gumbotil had been formed and before the invasion of the third ice sheet. As in the case of the Nebraskan drift and the Nebraskan gumbotil and related materials, Kansan drift and Kansan gumbotil constitute a second sedimentary unit of Glacial Time. Loess of late Yarmouth age is present in places in Illinois and Iowa.

The third cycle began with the recurrence of glacial conditions which resulted in the third ice sheet. In this third cycle the Illinoian drift was deposited during the glacial age of the cycle, and the Illinoian gumbotil and related weathered materials were developed by chemical action during the Sangamon interglacial age. Studies in Illinois and in southeastern Iowa have shown that the Illinoian gumbotil and related weathered materials are as widespread as are the poorly drained Illinoian uneroded uplands. The Illinoian gumbotil is not so thick as the gumbotil on the two older drifts. Before the close of the Sangamon interglacial age of the third cycle loess was deposited in places on the Illinoian gumbotil and elsewhere on the eroded surface of the Illinoian and older drifts. Where the late Sangamon loess was not deposited on the Illinoian gumbotil, gumbotil development was continuous throughout Sangamon time. The Illinoian drift and the Illinoian gumbotil and related weathered materials constitute a third sedimentary unit of Pleistocene time.

From the facts presented it is evident that in the first, second, and third cycles strikingly similar records were made. During each of the glacial ages drift was deposited, and during each of the interglacial ages the time was sufficiently long for the development of widespread gumbotil and related materials by physical and chemical weathering of the drift. Furthermore, during each of the interglacial ages where the conditions were unfavorable for gumbotil development related products of the profile of weathering were formed; erosion in places was effective, and in places the gumbotil and related weathered products were eroded before the end of the cycle. In some places sands and gravels and in other places loess were deposited during the interglacial ages, and in still other places, under favorable conditions, peat was formed. But the gumbotils, on account of their distinct characters, definite topographic positions, and distributions, have proved to be more useful than other criteria in differentiating and mapping the three oldest units of the Pleistocene. In other words, the gumbotils make possible the application of stratigraphic methods in correlating and mapping the three oldest drifts.

The fourth cycle includes the time from the beginning of the fourth ice sheet, the Wisconsin, to the present; it includes the Recent. During this

cycle several lobes of Wisconsin drift were left in the Mississippi Valley, and since their deposition relatively minor chemical changes have taken place in the deposits. The former Iowan glacial stage is now recognized to be the oldest of the substages of the Wisconsin stage. Later substages

TABLE 1.—*Classification of Pleistocene history in the Mississippi Valley*

Period (system)	Epochs (series)	Ages (stages)	Substages
	Eldoran	Recent	
Pleistocene or Glacial		Wisconsin	Mankato Cary Tazewell Iowan
	Centralian	Sangamon Illinoian	
	Ottumwan	Yarmouth Kansan	
	Grandian	Aftonian Nebraskan	

are the Tazewell, the Cary, and the Mankato. The widespread Peorian loess is related in origin and in age to the Iowan and Tazewell drifts and hence is intraglacial rather than a distinct interglacial deposit separating two drift sheets.

Since the retreat of the last Wisconsin glacial lobe—the Mankato lobe of the Wisconsin drift—the time has been so short that the chief evidence of chemical change has been the leaching of calcium carbonate of the Mankato drift to the depth of about 2½ feet. The depth of leaching of the Iowan or first lobe of the Wisconsin is about 5½ feet.

Pleistocene history has been interpreted to have involved four cycles. The first of these cycles included the Nebraskan glacial age and the Aftonian interglacial age and has been named the Grandian epoch. The second cycle included the Kansan glacial age and the Yarmouth interglacial age; the two constitute the Ottumwan epoch. The third cycle included the Illinoian glacial age and the Sangamon interglacial age—the Centralian epoch; and the fourth cycle included the Wisconsin glacial age and the Recent age—the Eldoran epoch. These names were chosen from localities where the materials of the different stages have been studied in all their relationships and where they have a real distribution. The main

features of the classification of Pleistocene history in the Mississippi Valley (Kay and Leighton, 1933) are shown in Table 1.

The name Peorian continues to be used in Iowa for the widespread loess which lies on the Iowan drift and around its borders, and beneath the Man-

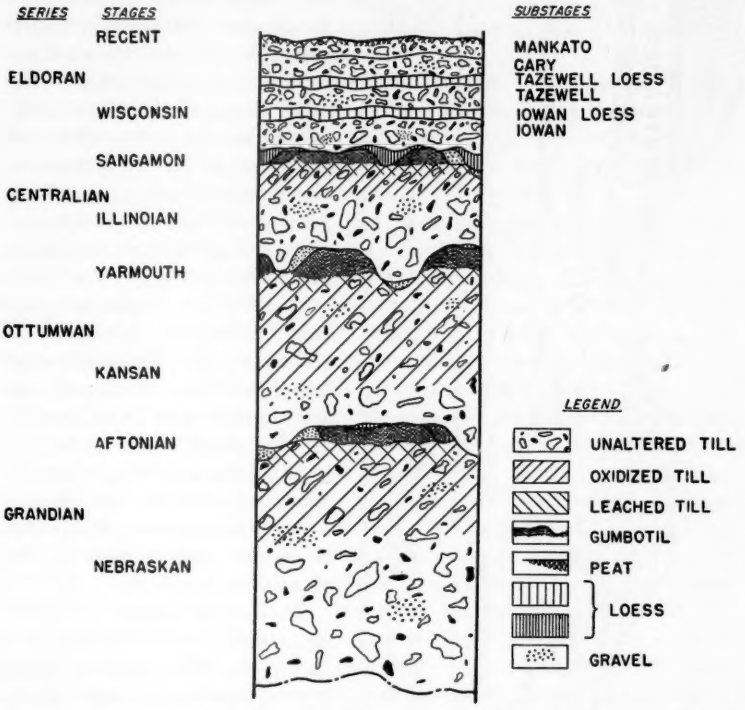

FIGURE 1.—*Diagrammatic section of glacial and interglacial materials of the Mississippi Valley*

kato drift; and in Illinois for the widespread loess which lies above the late Sangamon loess outside of the Tazewell drift. Within the borders of the Tazewell drift the loess, which immediately underlies it, is called Iowan, and the loess which overlies it, the Tazewell loess. These two loesses are indistinguishable outside of the Tazewell border and here compose the Peorian loess.

A comparison of the most recent interpretation of Pleistocene history with the interpretation of nearly 40 years ago shows no very distinct differences. The history involved in the two classifications of the Pleistocene

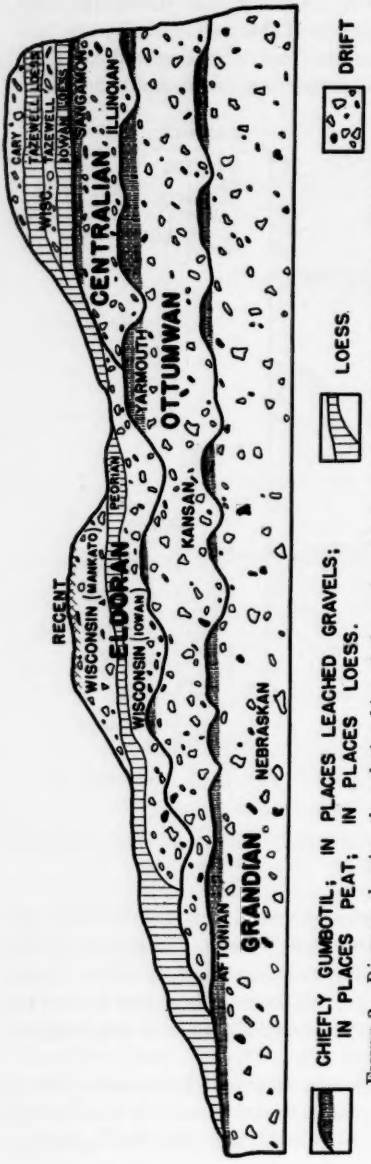

FIGURE 2.—*Diagram showing the relationships of the series and stages of the Pleistocene in the Mississippi Valley*

are alike in all major respects. This fact increases our admiration for the earlier workers in this field—Chamberlin, Leverett, Calvin, and their associates. In the year 1900 these geologists called the oldest drift pre-Kansan; it is now Nebraskan. The other stages of the two classifications are the same. The chief difference is in the interpretation of the fourth cycle. They recognized the close relationships in age between the Iowan and Wisconsin drifts and gave each of them a distinct stage rank; today the Iowan is included in the Wisconsin stage because evidence in Illinois and Iowa shows that from the beginning of the Iowan to the end of Mankato time there was continuous glaciation; the Peorian loess is intraglacial rather than a deposit made during a very short interglacial age.

The characteristics, distributions, stratigraphic relations, and mapping of the stages are much better established and understood at present than formerly as a result of the recent refinements of investigation. The late Sangamon loess has been recognized, and its relationships determined in recent years; the Recent is now considered to be a stage of the Pleistocene (Fig. 2).

The investigations of the Pleistocene record of areas other than the Mississippi Valley have

thrown much light upon the history of the glacial period more particularly with reference to the late Pleistocene—the Wisconsin and Recent ages.

DURATION OF PLEISTOCENE HISTORY

Does our present detailed knowledge of the origin and history of glacial and interglacial materials furnish an adequate basis for estimating the probable duration of Pleistocene time?

Many geologists have evinced a keen interest in the problem. The similarity of processes and products of the present day to those of immediately preceding ages of the earth's history, and the accessibility of the products and their comparatively slight alteration have invited repeated attempts to deduce time values in terms of years.

Permit me at this time to refer only to an estimate made by the writer in a paper published a few years ago (Kay, 1931, p. 452-461). The evidence used in reaching judgments as to the durations of the interglacial ages was gained chiefly from extensive field studies in Iowa of relative depths of leaching of calcium carbonate in similar materials which throughout their times of leaching were similarly situated topographically and climatically. Leached gravels of known ages were compared. The differences in depths of leaching are the results of the differences in lengths of time to which the gravels were subjected to weathering agents. The depth of leaching of upland gravels in the Mankato drift was determined to be about 2½ feet. This leaching is the result of exposure to weathering since the retreat from Iowa of the late Wisconsin ice sheet, that is, through a period estimated to be about 25,000 years. With this rate of leaching of gravels as a unit estimates were made of the lengths of time involved in the leaching of other gravels of known ages. The results for Iowa are as follows: Post-Iowan time, 55,000 years; Sangamon interglacial time, 120,000 years; Yarmouth interglacial time, 300,000 years; and Aftonian interglacial time, 200,000 years. The combined durations of Aftonian, Yarmouth, and Sangamon interglacial ages and of post-Iowan time total 675,000 years.

The durations of the glacial ages in Iowa were estimated from present-day consensus as to the rates of advance and retreat of ice sheets. For the retreat of the Mankato from Iowa the rate of 1 mile in 10 years was adopted. The same rate was assumed for the advance of this ice sheet into Iowa and also for the advances and retreats of earlier ice sheets. The minimum duration of glacial time in Iowa was calculated to be about 30,000 years. The years involved in the duration of Mankato glacial time, about 3000 years, are included in the 55,000 years of post-Iowan time.

In accordance with the methods of evaluation adopted minimum inter-

glacial and post-Iowan time in Iowa was determined to be approximately 675,000 years, and minimum glacial time in Iowa about 30,000 years, the combined estimates giving for the whole Pleistocene in Iowa a minimum duration of about 700,000 years. How far this estimate of the minimum duration of the Pleistocene falls short of the actual duration cannot be determined from reliable quantitative evidence. However, it would seem safe to state that the Pleistocene involved probably a million years, possibly twice this length of time.

EARLY MAN IN AMERICA

The Pleistocene record contains the chief story of the human species. From Java has come Pithecanthropus and related forms—fossil man in deposits which indicate an early Pleistocene age. From China has come Pekin man, Sinanthropus, resembling in some respects the Java man; from England the Piltdown man; from Germany Neanderthal man, and then from deposits of the last glacial age, in France, the Cro-Magnons, a superior race of men belonging to the modern species, Homo sapiens.

And what of early man in America? The evidence supports the view that man came into America from Asia by way of the Bering Sea. The question is, When did he come? This has been a controversial question for many years. In accordance with our present knowledge man had not arrived in America much before the close of the last continental ice sheet, the Wisconsin.

Most of the oldest geological records of man in North America have been found on the Great Plains and in the southwestern States. Cultures in Southern California, southeastern Arizona, and adjacent New Mexico may be considerably more than 10,000 years old. The Folsom deposits in New Mexico and Colorado are considered by geologists to be very late Wisconsin or early Recent in age. Terraces in Nebraska containing mammalian remains and artifacts have been interpreted to be late Pleistocene in age.

The skeleton of "Minnesota Man" has created, perhaps, more widespread interest than any other human remains in America. The investigations of Dr. Jenks, supported by the geological field studies of Stauffer, Thiel, Leighton, Bryan, MacClintock, and myself, warrant the judgment that the skeleton reached its position by natural causes. From all the geological evidence at hand it would appear that the skeleton may well be as old as the varved silts in which it was embedded. These silts are interpreted to be of a late Wisconsin age of the Pleistocene period and hence 18,000 to 20,000 years old. The skeleton, therefore, appears to rank among the oldest human remains thus far found in America.

CONCLUSION

As investigations of Pleistocene deposits continue into the future, better and better standards of measurements will be established, and hence more and more accurate interpretations of Pleistocene time will be made. Evidence will be found, no doubt, which will enable the causes of glacial and interglacial ages to be better understood than at present. The areas of Pleistocene age beyond the glaciated borders will be investigated more fully, and in connection with these studies no doubt clearer insight will be gained regarding man's relationships to this last chapter in the earth's history. The classification of the Period will probably have to be revised from time to time as further refinements of study of glacial and interglacial deposits enable more definite interpretations of their origin and history.

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